The Effects of Compulsory Schooling on Health and Hospitalization over the Life-Cycle

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October 24, 2022

Abstract

Despite serving as one of the more celebrated relationships in health economics, evidence on the relationship between education and health remains quite mixed–with limited research devoted to how these effects evolve over time. Leveraging a 1972 compulsory schooling reform within the United Kingdom, this paper examines the effects of education on health and health care utilization over the life cycle. Our regression discontinuity estimates suggest that while the reform did not produce meaningful changes in self-reported health, hospitalization among men decreased for admissions stemming from lifestyle-related conditions–with these effects varying heterogeneously over the life cycle.

Keywords: Health; Education; Compulsory Schooling; Life Cycle; Gender Differences;
JEL: I10; I12; I14; I20

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1 Introduction

Research examining the relationship between education and health occupies considerable space within the health economics literature. Indeed, a key prediction from the canonical Grossman (1972) model of the demand for health suggests that an increase in formal schooling can lead to improvements in health and quality-of-life through more efficient health production. Such efficiency gains can take the form of improved processing of health information and early adoption of health care innovations (Rosenzweig and Schultz, 1983; Kenkel, 1991; Glied and Lleras-Muney, 2008). While an extensive empirical literature has since emerged exploring the nature of this relationship (Grossman and Kaestner, 1997; Grossman, 2000, 2006; Cutler and Lleras-Muney, 2012), much of this work also highlights several important challenges to estimating the causal effect of education on health. Some of the well-noted challenges to identification include endogenous time preferences (Fuchs, 1982), simultaneity, and other forms of omitted variable bias (Grossman, 2015).

Noting many of the aforementioned challenges to identification, a growing literature has turned to compulsory schooling reforms in an effort to obtain causal evidence concerning the effects of education on a number of health outcomes (Adams, 2002; Lleras-Muney, 2005; Oreopoulos, 2006; Black et al., 2008; Mazumder, 2008; Albouy and Lequien, 2009; Chou et al., 2010; McCrary and Royer, 2011; Van Kippersluis et al., 2011; Clark and Royer, 2013; Wilson, 2017; Barcellos et al., 2018; Davies et al., 2018; Meghir et al., 2018). However, even studies invoking nearly identical identification strategies and research settings ultimately come to different conclusions regarding the effects of education on health. Several studies leveraging two notable twentieth century raising of the school leaving age (ROSLA) reforms within the United Kingdom (U.K.) find that these policies led to nearly half of the population receiving an additional year of education, but ultimately provide contrasting evidence on the effects of these changes on health. While these particular reforms do not appear to produce any meaningful changes in self-reported health (Clark and Royer, 2013), other work finds evidence of improvements in certain lifestyle outcomes such as diabetes and obesity (Davies et al., 2018; Barcellos et al., 2018, 2022). Thus, findings based on subjective health measures (e.g., self-reported health and health behaviors) appear to be in conflict with other evidence based on more objective health measures (e.g., blood pressure and body composition measurements taken by a health professional).

This paper reconciles some of the contrasting empirical evidence on education and health by leveraging a similar twentieth century ROSLA reform within the context of Scotland. As a constituent nation of the U.K., Scottish schools were required to raise the minimum school leaving age from 14 to 15 in April 1947 and 15 to 16 in September 1972. We specifically employ a regression discontinuity (RD) design in order to produce estimates of the effects of the 1972 ROSLA reform on health and health care utilization.[1] These methods rest on the credible identifying assumption that the 1972 ROSLA reform serves as the sole event driving variation in educational attainment for individuals born one month apart. Our RD estimates of the effects of compulsory schooling on educational attainment are strikingly similar to those based on the experiences of England and Northern Ireland (Oreopoulos, 2006; Clark and Royer, 2013).

In addition to possessing nationally representative survey data on self-reported health measures, Scotland also allows for novel insight into the education-health gradient through a nationally representative longitudinal dataset for a large (semi-) randomly selected sample containing linked information on health care utilization, cancer diagnoses (pre- and post-mortem), and drug use based on administrative records. These data not only provide us with a unique opportunity to examine both subjective and objective health outcomes within the same population, but also allow us to explore an important theoretical consideration scarcely discussed within the literature – the evolution of the relationship between education

[1] We dedicate our attention to the 1972 ROSLA reform, and not the 1947 reform, for two reasons. First, the 1947 ROSLA reform coincided with several changes to the education system–including the construction of new schools and a significant expansion in teacher hiring. Second, the age of the birth cohort affected by the reform also results in smaller sample sizes and related estimation challenges for our life cycle analyses. In any case, our first-stage estimates for the 1947 ROSLA reform are remarkably similar to the previous literature and our "static" results qualitatively similar to those based on the 1972 reform. We report these findings in the appendix for completeness.

and health over the life cycle (Galama and Van Kippersluis, 2019; Kaestner et al., 2020). If the marginal productivity of health investment changes over the life cycle, one might expect the effects of increased schooling on health and health care utilization to vary across different age profiles as well.

Similar to previous research, our RD estimates yield little evidence concerning the effects of the 1972 ROSLA reform on a variety of subjective health measures such as self-reported poor health status and smoking. These findings are in stark contrast to the corresponding OLS estimates which are generally large and in some cases possess the opposite sign. While these results suggest the absence of a strong relationship between education and health, our estimates based on administrative records indicate that the reform led to meaningful improvements in both health and health care utilization. We specifically find that an additional year of education reduces hospitalization episodes and stays. These effects are concentrated among men admitted to the hospital for conditions generally related to critical "lifestyle" behaviors–including cardiovascular disease and alcohol abuse.

This study also yields new insights into the evolving relationship between education and health over the life cycle. Consistent with the extended Grossman (1972) demand for health model introduced in Kaestner et al. (2020), the effects of the 1972 ROSLA reform on hospitalization indeed vary throughout the life cycle and across health conditions. For example, cumulative reductions in hospitalization for cardiovascular disease occur almost exclusively among men beginning in their late-30s and become more pronounced upon reaching the age of 55. While men also experience statistically significant reductions in injury-related admissions, these effects are relatively constant and more concentrated within the intermediate age range. Such findings are consistent with both a convergence in health stock and changes in the marginal productivity of health investment over the life cycle. Finally, an additional year of education also reduces the probability of a pre- or post-mortem cancer diagnosis. Our findings are not accounted for by selective mortality and collectively suggests that while the compulsory reforms fail to produce meaningful effects on self-reported measures of *overall* *well-being*, individuals subject to the reform indeed use fewer health care resources associated with poor health and health behaviors.

Our research contributes to the existing literature between education and health in several important ways. First, our comparison of subjective and objective health information reconciles strands in the literature on the effects of schooling on health which produce conflicting results. This is important because analyses of the education-health gradient that use school entry policies for identification have received immense attention, not least because the correlation between education and health, by way of comparison, dwarfs the effects of income or race. Second, rich panel data based on administrative health records permits new insights into a growing body of evidence on education and hospitalization (Arendt, 2008; Meghir et al., 2018). Third, we build on our hospitalization findings by showing how these effects vary over the life cycle and are heterogeneous across disease conditions. Research relying on aggregate estimates might not capture the significant, time-varying contributions of education to health during critical periods of health stock depreciation (Galama et al., 2018; Kaestner et al., 2020). Finally, our work also contributes to a broader conversation regarding contrasting evidence on the education-health gradients based on subjective and objective health measures (Clark and Royer, 2013; Davies et al., 2018; Barcellos et al., 2018, 2022). One potential explanation for these differences could be the failure of compulsory schooling to improve perceived overall well-being later in life. Another explanation involves the susceptibility of subjective health measures to measurement error and non-random differences in responses that challenge the interpretation of the empirical estimate of interest (Bound, 1991; Mackenbach et al., 1996; Bound et al., 1999; Johnston et al., 2009).

The remainder of the paper is organized as follows. Section 2 examines some theoretical evidence on the relationship between education and health over the life cycle. Section 3 provides an overview of the institutional setting and the nature of the ROSLA reforms in Scottish schools. In this section, we also outline our identification strategy. In Section 4, we introduce our data and discuss how these data differ from other sources used in this literature. We present our results in Section 5 along with robustness checks. Section 6 discusses the channels through which education affects health while Section 7 concludes.

2 Health and Education over the Life Cycle

One of the most celebrated predictions from the canonical Grossman (1972) demand for health model involves its ability to account for the well-documented education-health gradient. Within this framework, education contributes to the endogenous determination of health through its effects on the marginal productivity of health investment. More educated individuals achieve greater health stock for each unit of health investment. This investment could involve a variety of inputs working to augment optimal health stock–including improved dietary regimen or more sophisticated medical care consumption.

While many studies examining the education-health gradient often hold the productivity of these investments constant, there are several reasons why one might believe that this form of human capital investment can have varying effects on health over the life cycle. First, dynamic complementarities between skill and health capital might differ over the life cycle (Galama and Van Kippersluis, 2019). Second, the marginal productivity of health investment could differ considerably during early stages of the life cycle, often characterized by slower depreciation of health stock, and could even involve a different combination of inputs altogether relative to choices made later in life. For example, strength training and nutrition could be associated with greater health improvements earlier in life while annual medical examinations could yield greater benefits among middle-aged adults. In either case, any efficiency gains associated with education are likely to vary with age.

Kaestner et al. (2020) make this point more explicitly by first modifying the health production function within the Grossman model to allow for education to determine health through the productivity of health investment.

$$H_t = H\prod_{j=k}^{t-1}(1-\delta_j) + \alpha_0 I_0 \prod_{j=k+1}^{t-1}(1-\delta_j) + \cdots + \alpha_{t-1}(E)I_{t-1}(E) \tag{1}$$

where H_t is health at age t, I_{t-1} is gross health investment at age $t-1$, δ is depreciation of health stock, and α is the productivity of health investment. If education positively affects health through greater health investment productivity, equation (1) suggests that the effects of education are age-specific and will differ with changes in δ. Kaestner et al. (2020) further show that the cumulative effect of education on health is then given by:

$$\frac{\partial H_t}{\partial E} = \sum_{k}^{t-2}\left[\left(\frac{\partial \alpha_k}{\partial E}I_k + \alpha_k\frac{\partial I_k}{\partial E}\right)\prod_{j=k+1}^{t-1}(1-\delta_j)\right] + \frac{\partial \alpha_{t-1}}{\partial E}I_{t-1} + \alpha_{t-1}\frac{I_{t-1}}{\partial E} \tag{2}$$

Equation 2 suggests that the total effect of education at age t is given by the sum of its effects on both the productivity and quantity of health investment. The authors go on to show that while education only yields small effects on mortality through the age of 60 before reducing the hazard rate of death, its effects on morbidity are greatest between the ages of 45 and 60.

This extension of the Grossman model offers several compelling explanations for some of the contrasting findings on the effects of compulsory schooling reforms on health. Studies producing aggregate estimates across age group would capture neither age-specific changes in investment productivity α_t nor the subsequent changes in quantity of investment. For example, the effects of an additional year of formal schooling on health care utilization could be small at earlier ages only to become more pronounced later in life when health stock depreciation is more salient. Differences in follow-up periods across study samples could also yield conflicting evidence on the education-health gradient if investment productivity varies over the life cycle–with several studies based on the U.K. ROSLA reforms leveraging variation among similar populations albeit at different points in life (Davies et al., 2018; Barcellos et al., 2018, 2022). Finally, one might also expect for the effects of education on health to vary across the health conditions and behaviors under consideration. Each of these explanations suggest that varying investment investment productivity over the life cycle is critical to understanding the relationship between education and health.

3 Empirical Strategy

3.1 Minimum School Leaving Age in Scotland

Similar to the rest of Great Britain, Scotland experienced two ROSLA reforms throughout the twentieth century. Through the 1944 Education Act, Scotland first raised the minimum school leaving age from 14 to 15 in 1947 and did so again from 15 to 16 in September 1972. Thus, students born on or after April 1, 1933 were compelled to stay in school for one more year relative to their peers born before this date. Similarly, students born on or after September 1, 1957 were also compelled to stay in school for an additional year compared to other students born before this date. As noted in previous work, each of these U.K.-wide compulsory schooling reforms equally applied to England and Scotland (Buscha and Dickson, 2018).

Figure 1 illustrates the effects of each reform on Scottish educational attainment by quarter of birth. Since children typically start school upon reaching five years of age, the 1947 reform (indicated by the first vertical line) pushed the affected cohorts into obtaining at least 10 years of education. As a result, Figure 1 shows that the percentage of students who obtained nine years of education or less was cut by more than half. In the same vein, the 1972 reform dramatically reduced the number of students leaving high school with ten or fewer years of education.[2] The effects of each compulsory reform on Scottish educational attainment are strikingly similar to evidence based on other areas of the U.K. (Braakmann, 2011; Clark and Royer, 2013; Janke et al., 2020).

[2]Given that the September 1, 1972 implementation date is contained in 1957Q3, the full effect of the 1972 reform in Figure 1 does not materialize until the fourth quarter of that year.

We dedicate our attention to the effects of the 1972 ROSLA reform on educational attainment and health for two reasons. First, the 1947 ROSLA coincided with an ambitious educational infrastructure and teacher labor force expansion efforts after the Second World War known as the "Hutting Operation for Raising the School Leaving Age" (HORSA) program (Cowan et al., 2012). This program led to the construction of nearly 36,000 new school buildings and thousands of smaller dwellings known as "HORSA huts" by 1949. While any reduced-form effects associated with HORSA program likely operate through the significant increase in educational attainment, we cannot rule out the possibility that health improvements could be affected by changes in the quality of education provided through the first reform. These expansion efforts were largely complete before the 1972 ROSLA reform. Second, attrition among the 1933 birth cohort by the end of our 2016 sample period also makes robust estimation more challenging and life-cycle analyses less straightforward. Given these considerations, all subsequent analyses will focus on the 1972 reform.[3]

3.2 Estimation

Our identification strategy leverages exogenous variation in educational attainment through the 1972 ROSLA reform in order estimate the effects of education on health over the life cycle. The retroactive application of the reform specifically allows for a regression discontinuity (RD) design in which students born on or after September 1, 1957 were compelled to stay in school for an additional year relative to their counterparts born before the policy cutoff date. Previous studies examining the effects of the U.K. ROSLA reforms generally employ a fuzzy RD design of the following form:

$$\begin{aligned} H_{ict}^{a(t)} &= \delta_0 + \delta_1^{a(t)} E_{ic} + f(Run_{ic}) + \delta_2 \mathbf{X}_{ict} + \varepsilon_{1ict} \\ E_{ic} &= \alpha_0 + \alpha_1 D_{ic} + f(Run_{ic}) + \alpha_2 \mathbf{X}_{ict} + \varepsilon_{2ict} \end{aligned} \tag{3}$$

where $H_{ict}^{a(t)}$ is a measure of adult health or health care utilization for student i from birth

[3]For completeness, we report our main estimates based on the 1947 ROSLA reform in the appendix.

cohort c (i.e., month-year) at time t, E_{ict} is years of formal schooling (i.e., the age at which an individual leaves full time education minus five), D_{ic} is a binary indicator of being born on or after the cutoff date, and $f(Run_{ic})$ is a function of our birth month-year running variable centered around the reform cutoff date. For estimation based on our objective health measures, causal estimates of $\delta_1^{a(t)}$ are presented in aggregate and up to specific ages $a(t)$ over the life cycle. Estimation of (3) also includes a vector of covariates $\mathbf{X}_{ict}$ which flexibly controls for age, sex, birth month, and survey year. All estimates are obtained from local polynomial regression discontinuity (RD) estimation with automated bandwidth selection and bias-corrected inference (Calonico et al., 2014).

The causal effect of an additional year of education on health, $\delta_1^{a(t)}$, rests on the standard instrumental variables (IV) identifying assumptions. Similar to previous studies leveraging the U.K. ROSLA reforms, the natural experiment within Scotland also benefited from the retroactive application of the reform precluding sorting around the cutoff potentially driven by strategic parental fertility choices and enrollment.[4] As shown in Section 3.1, the dramatic decline in students finishing with no more than a 10^{th} grade education suggests a strong "first-stage" relationship between the 1972 ROSLA reform and educational attainment.

Table 1 serves as the regression analog to Figure 1 and formally shows the effects of the 1972 reform on years of formal schooling. Our RD estimates suggests that the 1972 reform increased average educational attainment by 0.42 years and decreased the probability of dropping out after 10 years of schooling by roughly 29 percentage points. These effects are similar for men and women, and more importantly, do not extend beyond the 11^{th} grade. Figure 2 provides graphical evidence of these findings and suggests that compliance with the law was quite strong. Moreover, we can also conclude that most students affected by the reform (i.e., the "compliers") would have dropped out of school otherwise. Both average

[4]We provide additional information on various predetermined characteristics and outcomes in Table A1 of the appendix. For example, corresponding Figure A3 shows no evidence of differences in the percentage of individuals who were raised Protestant. Past (and to some extent contemporary) ethnic conflict in Scotland proceeded primarily along religious lines in which Protestants of the Church of Scotland are the majority and Roman-Catholics, often of Irish descent, are a sizeable minority. We also find no evidence with respect to discontinuities in ethnic and gender composition.

levels of education, and our first-stage results, for Scotland are very consistent with findings from the existing literature based on the experiences of England and the rest of the U.K. (Clark and Royer, 2013; Buscha and Dickson, 2018; Janke et al., 2020).[5]

Due to data limitations that we outline in Section 4, we are unable to directly pursue the fuzzy RD approach specified in (3) and will instead take a "reduced-form" approach in estimating the causal effect of the 1972 ROSLA reform on health:

$$H_{ict}^{a(t)} = \beta_0 + \beta_1^{a(t)} D_{ic} + f(Run_{ic}) + \beta_2 \mathbf{X}_{ict} + \varepsilon_{3ict} \tag{4}$$

Given that our estimates are based on nationally representative data, we instead approximate the local average treatment effect (LATE) by re-scaling $\beta_1^{a(t)}$ by our first-stage estimate of α_1 from equation (3).[6] Within our context, the closeness of the LATE estimate to the population average treatment effect (ATE) also contributes to the external validity of our results (Oreopoulos, 2006).

4 Data

4.1 Scottish Longitudinal Study (SLS)

Our main data source is the Scottish Longitudinal Study (SLS) which links census data to administrative records. The Scottish census takes place every ten years and uses the same methodology developed by the Office for National Statistics (ONS) for the rest of the United Kingdom. The last census took place in 2011 and collected information on education, ethnic identity, religious identity, and housing for the entire Scottish population.

The SLS began in 2006 and created a longitudinal dataset for a representative subset of the population by linking these individuals across the 1991-2011 census waves. These

[5]First-stage estimates based on the U.K. Biobank project, in contrast, tends to over-sample more educated participants and subsequently find smaller effects of the 1972 reform on educational attainment (Davies et al., 2018; Barcellos et al., 2018, 2022)

[6]In fact, Abraham Wald (1940) famously showed that with a binary treatment variable and no control variables, δ_1 will be exactly equal to β_1/α_1.

individuals were selected by (semi-)randomly picking 20 of 366 possible birthdays. Any census participant born on one of these dates is automatically included in the SLS–yielding a 5.3% sample (approximately 270,000 individuals) of the entire Scottish population.

Detailed information on each participant's health and National Health Service (NHS) utilization records were matched to the SLS sample. In particular, we have information on all inpatient hospital admissions and discharges since 1981 including the type of admission. This information also contains the number of episodes corresponding to each admission. For example, a patient could be admitted as part of a consulting episode and before being transferred into a surgical unit–triggering a second episode within the same spell. We also observe the duration of each hospitalization episode in days. The SLS data also contains rich information on the main and secondary diagnoses for each admission as indicated by International Classification of Diseases (ICD) codes.[7] These ICD codes are used to categorize each inpatient episode by disease and injury type. Hospital episodes related to pregnancies and childbirth are excluded from the analysis. SLS participants are also linked to the Scottish Cancer Registry which includes information on all cancer diagnoses (including post-mortem).

The SLS data are uniquely qualified to address some outstanding questions on the causal effects education on health for several reasons. First, the SLS data are nationally representative and avoid many of the self-selection concerns associated with other data sources based on voluntary enrollment. Second, the SLS data allow for analyses of the effects of the 1972 ROSLA reform on more objective measures of health and health care utilization. These administrative records capture genuine differences in the demand for health and health care in adulthood while sidestepping some of the issues corresponding to self-reported health measures. The age profile of the 1957 birth cohort (i.e., ages 24-59) in our study also captures a

[7]Most diagnostic codes follow the 10^{th} ICD revision (ICD-10). Cases invoking the older 9^{th} revision (ICD-9) were converted to ICD-10. Some of the major classifications within our study include diseases of the circulatory system (ICD-codes I00-I99), respiratory system (J00-J99), digestive system (K00-K93), and several sub-categories such as heart disease (I00-I52), and liver disease (K70-K79). We also separately analyze hospitalizations related to risky health behaviors such as episodes related to alcohol poisoning (ICD-10 codes: T51, X45, X65, and Y15), intoxication and harmful use (ICD-10 codes: F10.0 and F10.1), alcohol dependency and withdrawal (ICD-10 codes: F10.2 to F10.9), and drug abuse related episodes (ICD-10 codes: T40 and T43.6; F11-F19). See Appendix B for additional details.

critical period of health production. Finally, the longitudinal elements of the SLS data also permit estimation of the effects of the reform on hospitalization over the life cycle. Previous work based on cross-sectional data sources either focus on the aggregate effects of the reforms within or across cohorts.

The SLS also takes several precautions with respect to selective sample attrition and retention. For example, immigration could present important challenges to our study design. Individuals who emigrated to Scotland later in life, but by virtue of their birth month-year would have been affected by the ROSLA reforms, could falsely be classified as "treated." Fortunately, the SLS is regularly cross-checked with NHS registrations. Since all residents are required, and possess a strong incentive to register with the NHS upon relocating in order to receive free universal health care, we can reliably assess their immigration status. In other words, we do not believe that left-censoring presents a meaningful challenge to our findings. As a precaution, we also limit our sample to individuals who were either born in the UK or arrived in the UK before turning 14 years old. Similarly, selective emigration could also lead to important empirical hurdles. However, emigrants are required to notify the NHS when they move abroad and these cases will be accounted for in the SLS data. Rare cases of emigration without notification can be detected by virtue of the census. Dedicated SLS staff examine the whereabouts of all participants who show up in one census wave but not the next. Deaths of individuals who were born on one of the 20 semi-randomly selected birthdays are also transmitted to the SLS by mortality registries.

The SLS also contains information on each participant's highest educational qualification such as O-Grades (equivalent to English O-Levels) or a university degree in addition to other basic demographic information typically surveyed within the census (e.g., age, sex, ethnicity, occupation, and post-code level of deprivation). However, one important limitation of the SLS education measures involves the absence of specific details concerning each participant's years of formal schooling. For this information, we instead turn to a secondary data source.

4.2 Scottish Health Survey (SHeS)

The Scottish Health Survey (SHeS) is a nationally representative survey and is primarily based on a personal interview. We specifically pool information from the 1995, 1998, 2003, 2008-2011, and 2012-2016 survey waves. The SHeS serves as the Scottish equivalent to the Health Survey of England (HSE) widely used elsewhere in the literature. One shared feature of the SHeS and HSE involves their very similar measures of self-reported health and health behaviors. One such question within the SHeS asks respondents to assess their health on a five-point Likert scale ranging from "very good" to "very bad." We group the bottom three categories of "fair", "bad", and "very bad" health into a single dichotomous indicator of "poor health." Survey participants also report whether they suffer from any longstanding illness, consume alcohol, or engage in any current or past smoking behaviors. We also code these subjective health indicators as dichotomous variables.

Crucially, the SHeS also contains information about the age at which a person left full-time education. In contrast to the SLS data, the SHeS allows us to calculate the number of years of schooling for each respondent. As shown in Figures 1 and 2, we use this information in order to construct our first-stage estimates describing the effects of the 1972 ROSLA reform on educational attainment. Our reduced-form estimates of the effects of the reform on health make use of the SHeS and SLS for our subjective and objective health measures, respectively. The fact that both data sources are nationally representative allows for us to carry out our reduced-form approach to estimating each LATE discussed in Section 3.2.

In order to illustrate the representativeness of both SHeS and SLS, we compare the one common education measure contained within both the SLS and SHeS describing whether a participant left full-time education without any qualification. Appendix Figure A2 demonstrates that in both the SLS and SHeS data the 1972 ROSLA reform reduced the percentage of the population that leaves school without any formal educational qualification by about four percentage points. This finding is comforting and suggests that these datasets are indeed comparable.

5 Results

5.1 Education and Self-Reported Health

As a first step in our investigation of the relationship between education and health, we turn to the subjective health measures within the pooled SHeS data. Table 2 presents our ordinary least squares (OLS) and reduced-form estimates for several self-reported (binary) health and health behavior outcomes. The OLS specification estimated here is comparable to the structural equation shown in (3) for which we use both years of schooling and a dummy variable for having more than 11 years of schooling as separate measures of educational attainment. We also provide separate estimates for both men and women.

Our OLS estimates generally confirm the familiar, positive relationship between education and health. For example, an additional year of schooling reduces the probability of reporting poor health by 3.5 percentage points and the prevalence of long-standing illnesses by 2.0 percentage points. Education also reduces the probability of being a past or present smoker. Interestingly, an additional year of education increases the likelihood of current alcohol consumption–a finding which could reflect changes in social circumstances that ultimately alter drinking behavior rather than serving as a sign of alcohol abuse (Huerta and Borgonovi, 2010). These effects are similar for both men and women. Specifications based on our dummy indicator for completing more than 11 years of schooling produce qualitatively similar, but large statistically significant effects for each of our outcomes as well.

In line with the literature on education and health, our reduced-form estimates demonstrate the well-documented issues of endogeneity among the OLS estimates. These reduced-form estimates, based on equation 4, appeal to exogenously induced changes in educational attainment due to the 1972 ROSLA reform. In other words, we ask whether the large increase in formal schooling associated with this reform led to meaningful changes in self-reported health and health behaviors. Table 2 suggests that this does not appear to be the case. With an exception for current alcohol consumption, none of our reduced-form estimates are statistically significant at conventional levels. Not only are these estimates smaller in magnitude relative to the corresponding OLS estimates, some results also possess the opposite sign. For example, the first column of Table 2 suggests that the 1972 ROSLA reform *increased* the incidence of poor health by 0.005 percentage points–although we cannot rule out the possibility of no effect or meaningful reductions either. These findings are also consistent for both men and women.

Graphical evidence within Figure 3 also provides no clear evidence of changes in the incidence of poor health or long-standing illness among month-year birth cohorts about the cutoff. As such, both our OLS and reduced-form estimates are strikingly similar those reported in Clark and Royer (2013) who invoke a nearly identical natural experiment resulting in extraordinary changes in educational attainment. While these subjective health measures of health and health behaviors indeed serve as important correlates of mortality, questions still remain as to whether any of these estimates ultimately capture changes in *objective* health improvements (e.g., physical health) later in adulthood. Therefore, we instead turn to our longitudinal sample of administrative health records in order to examine the effects of schooling on the demand for health and health care utilization in aggregate and over the life cycle.

5.2 Education and Objective Health

5.2.1 Inpatient Hospitalizations

Similar to other forms of medical care, a large literature exists on the contributions of hospital care quality and spending to health (Allison et al., 2000; Barnato et al., 2010; Romley et al., 2011; Doyle Jr et al., 2015; Skinner and Staiger, 2015; Cutler et al., 2019). Within the context of the Grossman model, inpatient care serves as one potential input in the production of health. Any effects associated with the 1972 ROSLA reform could reflect an important differential between changes in gross investment and health stock among the more educated (Grossman, 2000). Thus, cohorts affected by the reform could ultimately demand more health and less inpatient care given any subsequent efficiency gains associated with health investment.[8]

Table 3 formally investigates the aggregate effects of the 1972 ROSLA reform on the demand for inpatient care over the 1981 to 2016 period by sex. Panel A shows that men affected by the reform, on average, experienced 1.6 fewer inpatient hospitalization episodes than cohorts born before the cutoff date. This point estimate implies nearly a 0.16 standard deviation decline in inpatient episodes and is statistically significant at conventional levels. We find an effect of comparable magnitude when instead using the number of hospital stays and a slightly smaller estimate when expressed in terms of inpatient care days. Our results are also suggestive of small declines in inpatient care utilization among women affected by the reform–although the standard errors are too large to rule out a null effect.

Note that each of the coefficients shown in Table 3 are reduced-form estimates. As discussed in Section 3.2, the corresponding LATE estimates can be obtained by scaling each of these reduced-form coefficients by the first-stage estimates shown in Table 1. Given that the 1972 reform led to a 0.44 year increase in formal schooling, our results indicate that an additional year of education reduces the number of inpatient episodes by almost half a standard deviation–a large and economically significant reduction. These findings are also interesting when compared to some of the conflicting evidence based on other educational reforms (Arendt, 2008; Meghir et al., 2018).[9]

While our flexible control for birth year-month should account for any differences in age across cohorts, we also explicitly test for age-specific effects of education by limiting our sample to individuals born between September 1951 and August 1963. These restrictions lead to the youngest person in our sample being 53 years old in 2016 when our study period

[8]Technically speaking, this specific relationship between education and medical care utilization holds if the elasticity of the marginal efficiency of capital (MEC) curve is less than one (Grossman, 2000) We further explore the extent to which this relationship holds over the life cycle in Section 5.3.

[9]For example, Meghir et al. (2018) finds that the gradual phase-in of the 1949-62 Danish compulsory schooling reforms did not produce any meaningful changes in hospitalization days in adulthood. However, these estimates are based on the 1940 through 1957 birth cohorts and subsequently reflect the hospitalization experiences of much older individuals as seen in the higher average number of hospital days within their sample.

ends and the oldest person 29 years old in 1981 when our inpatient data begin. In other words, we observe hospitalization records between the ages of 29 and 53 for every person in this sub-sample–allowing us to asses the effect of the 1972 ROSLA reform on inpatient admissions for a fixed age range. Panel B of Table 3 shows that our results are robust to these age-specific sample restrictions. The reform reduced the number of inpatient episodes and stays experienced by men, between ages 29 and 53, by roughly 0.16 standard deviations. The point estimates for women are again smaller and only borderline statistically significant. Taken at face value, these results suggest that the compulsory schooling reform reduced the number of inpatient episodes between ages 29 and 53 by approximately 0.1 standard deviations for women.[10]

Overall, our analysis of inpatient hospitalization suggests that education reduces the number of inpatient hospitalization episodes with men in their younger years showing the largest and most significant improvements in health. The graphical evidence in Figure 4 supports our regression results. Panels (a), (c), and (e) show a clear drop in the number of inpatient episodes, stays, and days for men who were just about affected by the 1972 reform. For women of the same cohort, on the other hand, these drops are much less clear and indicative of very small effects at best.

We also consider the effects of the compulsory schooling reform on hospitalization by main diagnosis. Figure 5 shows the standardized reduced-form effects of the reform on inpatient care episodes across 21 health conditions and categories. Many of the improvements in hospitalization rates that we observe are primarily driven by improvements in cardiovascular health, most notably heart disease, and internal health complications related to the digestive system (e.g., intestines, gall, biliary, pancreas, and stomach). Hospitalization for some of these conditions can be rather acute and are often associated with certain "lifestyle" behaviors (Arendt, 2008). The same figure also confirms the substantial gender differences in the health returns shown in Table 3. Taken together, these findings suggest that the 1972

[10]The corresponding results for the 1947 ROSLA cohorts are shown in Appendix Tables A3 and A4 as well as Figures A4 and A5 and are also suggestive of health improvements for men.

ROSLA reform led to significant reductions in hospitalization among Scottish men.[11]

5.2.2 Alcohol and Drug-Related Hospitalizations

Excessive alcohol consumption, chronic drug use, and smoking serve as important choice variables in health production (Grossman, 2000). In many respects, hospitalization driven by these health behaviors can convey important information regarding underlying differences in gross health investment or the willingness to participate in certain risky behaviors (e.g., binge drinking) (McGeary and French, 2000; Marcus and Siedler, 2015). Within the context of our study, alcohol and drug-related hospitalization outcomes go beyond self-reported participation measures and instead reveal realized differences in deleterious behaviors serious enough to require inpatient care.

Table 4 leverages our hospitalization data in order to closely investigate the nature of this relationship within the context of the 1972 ROSLA reform. We specifically group inpatient hospitalizations due to alcohol poisoning, intoxication and harmful use, or alcohol dependency and withdrawal into a single category–using the number of episodes, stays, and days of inpatient care that are related to this category as our outcome. Our findings suggest that the health benefits of education, in the form of lower alcohol abuse rates, again accrue primarily to men. An additional year of education produces roughly a 0.29 standard deviation reduction in the number of inpatient episodes related to excessive alcohol consumption. Again, the differences across gender are remarkable as we fail to find any such pattern for women. The graphical evidence of Figure 6 corroborates this finding. Panel (a) displays a drop in the number of alcohol-related inpatient episodes for men born around the September 1957 cut-off date whereas no such drop can be found for women.

We also analyze the effect of education on inpatient hospitalization indicative of acute drug abuse. However, we note that these hospitalizations are very rare and this subsequently influences the precision of our estimates. With this caveat in mind, columns (5) and

[11] Appendix Figures A7 and A8 show the corresponding results when using inpatient stays and days instead of episodes.

(6) document a negative relationship between ROSLA exposure and drug-related hospitalizations for men. While this estimate is not statistically significant, we view this finding as potentially suggestive evidence of a negative effect of education on drug abuse.[12]

5.2.3 Cancer Diagnoses

We also examine the effects of the 1972 ROSLA reform on cancer diagnoses. Table 5 provides our reduced-form estimates of the effects of the 1972 ROSLA reform on the probability of receiving a cancer diagnosis for both men and women. We again find that these effects are primarily concentrated among men with the point estimate in column (1) suggesting a highly significant 4.8% reduction in the probability of receiving any cancer diagnosis within this group. Cancers of the urinary tract appear to play a salient role in determining the incidence of these diagnoses. We also find weaker evidence of declines in the probability of receiving a lung or genital cancer diagnosis among women exposed to the reform. Given that this cohort is only around 60 years old by the end of our observed sample period, these results are best interpreted as a potential reduction in the early onset of cancer.

While the reductions in cancer incidence associated with the 1972 ROSLA reform could reflect genuine health improvements, these effects could also be driven by changes in the composition of inputs employed in health production. For example, more educated individuals could demand more annual cancer screenings and check-ups (Palme and Simeonova, 2015). However, this behavior would generally imply a higher rather than lower incidence of cancer diagnoses to the right of the cut-off. Furthermore, the Scottish Cancer Registry also includes post-mortem cancer diagnoses which would, if anything, attenuate the estimates shown in Table 5.[13] Of course, cancer diagnoses are arguably more prone to measurement error than hospitalizations so these results are best interpreted as suggestive evidence.

[12]Given that drug abuse related hospital admissions are very rare, and due to disclosure reasons, we were required to group men and women together for the graphical results shown in Appendix Figure A10.

[13]Post-mortem cancer diagnoses account for just 1.4% of entries. All results are robust to excluding these detections.

5.3 Life Cycle Effects

Each of the reduced-form estimates shown thus far, demonstrating the effects of the 1972 ROSLA reform on health in adulthood, reflect aggregate effects over the life cycle. However, we might view the marginal productivity of health investment as being age-specific in the presence of depreciation in health stock later in life. In order to formally address this question, we leverage the inherent panel dimension of the SLS data and estimate the effects of the compulsory schooling reform on hospitalization through each observed age (e.g., the number of inpatient episodes by age 40) over more than two decades. More formally, we use equation 4 to provide dynamic reduced-form estimates $\beta_1^{a(t)}$ through each age $a(t)$. We then plot the standardized point estimates and 95% confidence intervals separately by sex.

Figure 7 presents our evidence concerning the effects of the 1972 ROSLA reform on inpatient hospitalization episodes over the life cycle in total and across various health conditions. Panel (a) shows our results for total inpatient hospitalization episodes, and while both men and women appear to benefit from earlier human capital investment through the reform, the sharpest reductions primarily occur among men beginning in their early 40s. These benefits persist through men's 40s and 50s while women enjoy substantially smaller health benefits that improve more slowly over the life cycle. Thus, Figure 7a confirms our qualitative understanding of gender differences in the education-health gradient while also articulating important age-specific effects as conceptualized by the extended Grossman model.

One might suspect that the evidence shown in Panel (a) simply reflect educational differences in inpatient care utilization rather than genuine improvements in physical health. Panels (b) through (d) of Figure 7 show that this is indeed not the case as the life cycle effects of the reform differ considerably across inpatient care diagnoses. For example, Scottish men affected by the reform experienced extraordinary reductions in inpatient admissions related to cardiovascular health. These improvements in hospitalization appear as early as men's late 30s and grow to a nearly -0.2 standard deviation reduction by their late 50s. Women do not appear to share in these improvements in cardiovascular-related admissions. In contrast, Panel (c) provides evidence of significant declines in hospitalization associated with the digestive system for both men and women over the life cycle–although the point estimates for men are twice as large upon reaching their mid-40s.

While the reduced-form effects for cardiovascular and digestive health admissions evolve similarly over the life cycle, the age profile for injury-related hospitalization effects remains fairly flat. Panel (d) suggests that men affected by the 1972 ROSLA reform experienced approximately a -0.1 standard deviation decline in injury-related hospitalization. These point estimates are salient, and statistically significant, beginning in men's mid-30s up until their late-40s. Explanations based on educational differences in workplace exposure (e.g., through an income effect) or even allocative efficiency could potentially account for the age profile of these effects (Grossman, 2000, 2015). We do not observe any clear evidence of changes in injury-related hospitalization for women.

Overall, the effects of an additional year of education on hospitalization are both age- and condition-specific. Similar to our aggregate reduced-form estimates, the health benefits of the reform are primarily concentrated among men with an exception for hospitalizations related to the digestive system. Heterogeneous effect sizes and age profiles also suggest that the reform produced important changes not only in health care utilization, but also in health given that many of the well-known epidemiological characteristics of cardiovascular disease generally emerge within populations in this intermediate age range.

5.4 Robustness Checks

5.4.1 Education and Mortality

One potential concern for our analysis involves the possibility of selective mortality. For example, higher mortality among men affected by the reform, relative to women, could also produce gender differences in hospitalization. This "survivorship bias" essentially could be driven by the least healthy men subject to the 1972 ROSLA experiencing relatively higher mortality–resulting in a less straightforward comparison. Education-induced mortality reductions could also account for differences in health outcomes across cohorts. If an additional year of education improves the longevity of a cohort's least healthy members, we would underestimate the effects of the compulsory schooling reform on health and health care utilization (Barcellos et al., 2018, 2022).

We investigate this issue using both population data from the 1991 Scottish Census in addition to population mortality records from National Records of Scotland. Since both data sources contain counts by month and year of birth, we can combine them into a single individual-level panel dataset in order to examine whether the 1972 ROSLA reform led to any significant changes in mortality. Following previous work (Sullivan and Von Wachter, 2009; Clark and Royer, 2013), we assess whether the reform produced a discontinuous change in mortality using a two-step estimation procedure over the 1991-2016 period. We first estimate a panel logit model in which the probability of dying in each month t for individual i of cohort c is a function of a full set of month-year cohort dummies, θ_c, as well as age fixed effects δ_a:

$$P(Death_{ict}|\theta_c, \delta_a) = F(\theta_c + \delta_a) \tag{5}$$

Using estimates $\hat{\theta}_c$ from (5) as our dependent variable, the second step involves estimating a local linear model of the following form:

$$\hat{\theta}_c = \pi_0 + \pi_1 D_c + f(Run_c) + \gamma_m + \epsilon_c \tag{6}$$

where γ_m is a set of calendar-month fixed effects. Intuitively, $\hat{\theta}_c$ measures the effect of being born in a particular cohort on the log odds of death. Equation (6), in turn, assesses whether a significant change in these effects takes place for cohorts born around the time of the compulsory schooling reform. Such a discontinuous change in mortality would be captured by π_1. We estimate (6) using weighted least squares where the weights are given by the inverse of the standard errors from estimation of (5). We also cluster our standard errors at the cohort level.

We present our findings from this analysis in Figure 8 which reveals no evidence of a discontinuous jump in the log odds of death for cohorts affected by the reform. These point estimates are economically small, -0.019 for men and -0.002 for women, and statistically insignificant at conventional levels. The sign for each of these estimates also suggests that any effects on mortality would work in the opposite direction of any survivorship bias in our main health estimates. Similar to Clark and Royer (2013), we also fail to find any significant evidence of mortality effects associated with the 1972 ROSLA reform.

5.4.2 Selective Migration

We also examine whether selective migration could play a role in determining our results. SLS records contain information on both the country of birth and year of immigration, if applicable. This information allows us to reliably identify immigrants within these data and potential exposure to the compulsory education reform. However, this does not rule out the possibility that education selectively increases the propensity to emigrate. As mentioned in Section 4, U.K. residents are required to notify the NHS when they move abroad.[14] We account for potential selective migration by estimating the effects of the 1972 ROSLA reform on the probability of reporting such a relocation.

Appendix Table A7 provides no evidence that an additional year of education led to meaningful changes in emigration. These point estimates are both small in magnitude and statistically insignificant at the five percent level–suggesting that any potential bias arising from selective migration would be negligible. In particular, men affected by the reform are 0.009 percent more likely to report having emigrated in the SLS. Assuming that healthier individuals are more likely to emigrate (Farré, 2016), any bias attributable to selective migration would attenuate our estimates of the 1972 ROSLA reform on health and health care utilization.

[14] Pre-Brexit, pensioners moving to southern Europe, in particular, had a strong incentive to do so in order to claim entitlement to reciprocal healthcare in their host country.

5.4.3 Placebo Tests

Finally, we subject our principal findings to several falsification tests. For this exercise, we restrict our sample to SLS participants in our data who were born between September 1958 and August 1972. By virtue of their late birth years, none of these cohorts would have been affected by the 1972 ROSLA reform. For our test, we create a placebo treatment indicator equal to one for all SLS participants born on or after September 1965 and zero otherwise. As a hypothetical intervention, a null effect would lend credibility to 1972 ROSLA reform as a natural experiment while a significant effect on education or hospitalization would complicate our interpretation of the results. Appendix Table A8 indeed confirms the absence of any statistically significant placebo effect with respect to both education and inpatient admissions. Consistent with the growing literature on the U.K. ROSLA reforms, these findings further validate these compulsory schooling reforms as a credible natural experiment.

6 Discussion

Our results show that the 1972 ROSLA reform led to significant reductions in the demand for inpatient care among middle-aged men for health conditions often associated with improved lifestyle behaviors. A growing literature based on more objective health measures indeed finds that an additional year of education results in significant improvements in conditions such as obesity and supports theoretical claims of greater efficiency in health production among the more educated (Barcellos et al., 2018, 2022). The reductions we observe in alcohol and cardiovascular-related hospital admissions among men are consistent with evidence from studies leveraging other educational reforms and their effects on hospitalization (Arendt, 2008). Finally, we also provide new evidence demonstrating that the effects of education on health vary considerably over the life cycle. These hospitalization effects generally possess distinct age profiles across primary diagnoses. Thus, our evidence suggests that the more educated either enjoy more efficient health production or employ distinct input mixes when confronting critical periods of health stock depreciation later in life (Kaestner et al., 2020).

We also find consist evidence of gender differences in the health returns to education. Many of the aforementioned life style effects are not present among women in aggregate or across the life cycle. An extensive health literature suggests that women generally enjoy a longer life expectancy relative to men–although the exact mechanisms driving these differences are still not fully understood (Lawlor et al., 2001; Barford et al., 2006; Beltrán-Sánchez et al., 2015). The extended Grossman model offers some additional insight into these gender differences primarily through through two channels. First, women might possess distinct biological advantages which allow for them to enjoy either higher levels of initial health stock or health stock which depreciates more slowly (Austad, 2006; Cullen et al., 2016). The "female advantage" could also emerge through differential responses to public health interventions or differences in other environmental exposures (Goldin and Lleras-Muney, 2019). These differences, in turn, are likely to translate into gender differences in the education-health gradient.

Alternative mechanisms such as an income effect or other population changes could also contribute to the effects of the ROSLA reform on health (Grossman, 1972, 2000; Oreopoulos, 2006; Grossman, 2015). Previous work links the ROSLA reform effects to increased earnings. In particular, the gender differences we uncover are consistent with results by Devereux and Hart (2010) who document that the first British compulsory schooling reform yielded no returns for women and boosted male earnings by 3-7%.[15] While neither the SLS nor the SHeS contain information on earnings, the SLS records neighborhood characteristics. More specifically, our data contain information on the Scottish Index of Multiple Deprivation (SIMD). To calculate the SIMD, Scotland is divided into 6,976 small areas (known as "data zones") which are then ranked by levels of deprivation. Income is an important input for

[15] Recent work by Barcellos et al. (2021) documents further heterogeneity in wage gains by socio-economic background.

these calculations in addition to employment, education, health, access to services, crime, and housing. Put differently, SIMD is a comprehensive measure of respondents' socioeconomic environment which may well have a larger impact on health than a pure earnings measure.

In Table 6, we define as our outcomes of interest whether an individual resided in one of the 10% most deprived (i.e., "Most Deprived") and 10% most affluent (i.e., "Least Deprived") data zones as per the SIMD in either the 1991, 2001, or 2011 census. We also construct a measure for whether a SLS respondent resided in a data zone with below-median levels of deprivation. We find no meaningful evidence of changes in neighborhood quality for men and only suggestive evidence for women affected by the reform who are approximately 4.2 percentage points more likely to live in a neighbourhood with below-median levels of deprivation. Thus, the compulsory schooling reform did not produce any notable changes in the probability of relocating to a substantially more affluent neighborhood later in life.

The census-component of the SLS also contains information on occupations. We classify respondents as "skilled" if they report employment in professional, managerial, technical, and other forms of skilled occupations.[16] The first column of Table 6 shows little in the way of an effect of the ROSLA reform on occupational choice in adulthood. Admittedly, both our deprivation and occupational proxies are too crude to fully discount the role of a socioeconomic channel.[17] At the same time, our study takes place in a context in which a critical input within the health production function, health care, is freely and universally accessible through the NHS. Of course, financial resources still play an important role in the acquisition of other health inputs. However, the effects of the ROSLA reform on health and health care utilization generally take place well into the prime age working years of our population of interest.

Similar to previous literature, we also find that the compulsory reforms generally did not lead to meaningful changes in subjective health. Individuals who experienced the reform were

[16] These other forms of skilled occupations include both skilled manual and non-manual occupations (as opposed to "partly skilled" or "unskilled occupations").

[17] Another potential mechanism involves ROSLA-induced changes within marriage markets (Hener and Wilson, 2018; Anderberg et al., 2019).

statistically no less likely to report being in poor health, experience a longstanding illness, or possess current (or past) smoking habits. These findings are very much consistent with the subjective health estimates from Clark and Royer (2013) and suggest that the U.K. ROSLA reforms did not improve self-reported health or health behaviors. The sole exception among our subjective health estimates involves individuals receiving an additional year of education also being 1.7 percentage points more likely to report being a current drinker. In contrast, men who received an additional year of education experienced a 0.11 standard deviation reduction in alcohol-related inpatient episodes. Given that casual alcohol consumption does not necessarily imply poor health behavior, one possibility could be that men affected by the reform were simply more likely to report their drinking status and less likely participate in excessive alcohol consumption behavior that might require future hospitalization.

How exactly should we reconcile the differences in self-reported health and our findings on health care utilization? Our findings on alcohol-related consumption and hospitalization suggests that systematic differences might exist in how survey participants report their drinking behaviors along the lines of education. Unlike self-reported mental health measures, which are congruent with clinical diagnostic procedures that already rely upon self-reported behavior, self-reported physical health measures entertain a variety of perceived health improvements–including changes in overall well-being (Finkelstein et al., 2012). Thus, an alternative interpretation of these subjective health findings could involve the reform having limited effects on overall well-being rather than failing to demonstrate significant improvements in actual physical health. A notable example of divergence in subjective and objective health measures comes from Johnston et al. (2009) who compare self-reported measures of hypertension with blood pressure measurements taken by a nurse practitioner. The authors find that 85% of their sample engaged in false negative reporting of hypertension with lower income households being significantly more likely to provide a false negative report. Systematically biased measures of self-reported health could similarly result in attenuated estimates of the effects of education on health (Bound, 1991).

7 Conclusion

In this paper we combine longitudinal, nationally representative administrative data with a natural experiment generated by the 1972 British compulsory schooling reform in order to shed new light on the relationship between education and health. Our regression discontinuity design also reconciles some of the conflicting results within this literature. Similar to previous studies, we find no effect of additional schooling on self-reported health measures as obtained from health surveys. In contrast, we show that education reduces costly hospitalization related to certain lifestyle behaviors and lower cancer incidence. These health benefits are most pronounced for men and for conditions related to digestive and cardiovascular diseases.

We also document that the largest education-induced improvements in health occur early on in the life-cycle during men's thirties and forties and persist until their early fifties. Higher mortality or selective out-migration do not drive our results. While we certainly acknowledge other avenues exist through which education might improve health, we also provide evidence that the positive effect of education may operate by way of a behavioral channel (e.g., lower rates of alcohol abuse).

We also note that our study identifies a local average treatment effect (LATE) of an additional year of high school education for those individuals who were prompted to stay in high school for an additional year. While strong compliance ensures that our LATE resembles the average population effect, it is not informative with respect to the health effects of, say, an additional year of tertiary education. This seems acceptable not least because recent increases in compulsory schooling age predominantly affects high school education. Therefore, our results inform the greater policy discussion surrounding ROSLA reforms with the caveat that we cannot distinguish between the effects of increases in education quality or even (potentially health-related) content covered in additional schooling time.

Finally, our research highlights additional points of emphasis for future research. We show that the use of arguably more objective, administrative data can lead to substantially different conclusions than those drawn from survey data. This issue, in addition to the potential for non-response bias in surveys, was recently highlighted by Dutz et al. (2021). We also find evidence of substantial, and persistent, gender differences in the relationship between education and health. Differences in the effects of the compulsory schooling reforms are quite pronounced not only in aggregate, but also over the life cycle and provide further evidence that mechanical changes in health stock deterioration cannot adequately explain our results. Furthermore, our evidence lends an additional causal interpretation to earlier work presented in Cutler and Lleras-Muney (2008) who conclude that the health-education gradient peaks around the ages of 50–60. Life cycle effects are thus a phenomenon that deserves further study in future research by leveraging high quality panel data.

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Tables and Figures

Table 1: First Stage Results: Effect of 1972 Compulsory Schooling Reforms on Education

	Years of Education	≤9 years	≤10 years	≤11 years	≤12 years	≤13 years
	All (N=66,655; bandwidth = 42 months)					
Estimate	0.415***	−0.019***	−0.286***	−0.014	−0.023	−0.025
	(0.103)	(0.005)	(0.015)	(0.023)	(0.020)	(0.020)
Outcome Mean	11.98	0.015	0.336	0.593	0.726	0.782
	Men (N=29,390; bandwidth = 54 months)					
Estimate	0.443***	−0.024***	−0.283***	−0.018	−0.029	−0.028
	(0.113)	(0.008)	(0.021)	(0.029)	(0.026)	(0.021)
Outcome Mean	12.03	0.017	0.343	0.596	0.707	0.771
	Women (N=37,265; bandwidth = 42 months)					
Estimate	0.435***	−0.016***	−0.284***	−0.014	−0.027	−0.026
	(0.140)	(0.006)	(0.016)	(0.029)	(0.027)	(0.027)
Outcome Mean	11.95	0.014	0.331	0.591	0.740	0.790

Notes: ∗ ∗ ∗/ ∗ ∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the first stage effects of compulsory schooling reforms on educational attainment, using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table 2: Effect of Education on Self-Reported Health - 1972 ROSLA

	Poor Health	Illness	Current Drinker	Current Smoker	Ever Smoked
	All (N=66,655; bandwidth = 79 months)				
Mean	0.273	0.454	0.893	0.300	0.603
(SD)	(0.446)	(0.498)	(0.309)	(0.458)	(0.489)
Reduced Form	0.005	0.008	0.017*	-0.022	0.018
	(0.012)	(0.014)	(0.010)	(0.017)	(0.017)
OLS (years of schooling)	-0.035***	-0.020***	0.010***	-0.045***	-0.033***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
OLS (>11 years)	-0.155***	-0.082***	0.046***	-0.196***	-0.145***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
	Men (N=29,388; bandwidth = 75 months)				
Mean	0.241	0.422	0.914	0.287	0.606
(SD)	(0.428)	(0.494)	(0.280)	(0.453)	(0.489)
Reduced Form	0.035	0.021	0.008	-0.014	0.024
	(0.026)	(0.025)	(0.016)	(0.027)	(0.027)
OLS (years of schooling)	-0.033***	-0.019***	0.008***	-0.040***	-0.026***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
OLS (>11 years)	-0.148***	-0.072***	0.035***	-0.174***	-0.118***
	(0.005)	(0.006)	(0.004)	(0.005)	(0.006)
	Women (N=37,262; bandwidth = 71 months)				
Mean	0.297	0.479	0.877	0.309	0.601
(SD)	(0.457)	(0.500)	(0.328)	(0.463)	(0.490)
Reduced Form	-0.027	-0.020	0.021	-0.015	0.014
	(0.017)	(0.019)	(0.014)	(0.016)	(0.020)
OLS (years of schooling)	-0.037***	-0.021***	0.011***	-0.048***	-0.039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
OLS (>11 years)	-0.162***	-0.090***	0.054***	-0.212***	-0.168***
	(0.005)	(0.006)	(0.004)	(0.005)	(0.006)

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the the effects of education on self-reported health outcomes. OLS estimates refer to the effect of an additional year of schooling or of having more than 11 years of education. Reduced from effects yield the effect of the compulsory schooling reforms on educational attainment and are obtained using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table 3: Effect of 1972 Compulsory Schooling Reform on Inpatient Hospitalizations

Panel A: All Hospitalizations

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	5.164	4.449	13.939	6.137	5.561	14.282
(SD)	(9.828)	(8.496)	(57.567)	(10.205)	(8.984)	(48.675)
Coef	-1.607^{**}	-1.322^{**}	-5.187^{***}	-0.632	-0.515	-1.566
(SE)	(0.714)	(0.605)	(1.799)	(0.438)	(0.406)	(2.138)
N	32,164	32,164	32,164	32,103	32,103	32,103
Bandwidth	14.61	15.21	15.77	28.42	23.99	25.97

Panel B: Hospitalizations between Ages 29 and 53

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	2.773	2.508	5.620	3.456	3.250	6.568
(SD)	(6.484)	(5.912)	(27.181)	(6.165)	(5.625)	(21.115)
Coef	-1.026^{**}	-0.870^{**}	-2.871^{***}	-0.553^{*}	-0.538^{*}	-0.703
(SE)	(0.399)	(0.349)	(0.824)	(0.307)	(0.279)	(0.667)
N	30,869	30,869	30,869	31,094	31,094	31,094
Bandwidth	16.16	17.10	14.37	20.65	23.52	15.65

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1972 compulsory schooling reform on the number of inpatient hospitalization as outlined in equation (4), using local polynomial regression discontinuity estimation. In Panel A, the outcome is the number of hospitalizations at any age between 1981 and 2016. In Panel B, the outcome is the number of hospitalizations between ages 29 to 53. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table 4: Effect of 1972 ROSLA on Alcohol and Drug-Related Hospitaliations

	Alcohol-Related Inpatient Admissions				Drug-Related Inpatient Admissions			
	Men		Women		Men		Women	
	Episodes	Days	Episodes	Days	Episodes	Days	Episodes	Days
Mean	0.311	1.03	0.128	0.412	0.094	0.034	0.068	0.028
(SD)	(2.019)	(9.638)	(1.029)	(4.968)	(2.114)	(0.467)	(1.543)	(0.352)
Coef	-0.213^{**}	-0.779^{**}	-0.004	0.051	-0.035	-0.037	-0.002	0.039
(SE)	(0.103)	(0.326)	(0.036)	(0.198)	(0.027)	(0.084)	(0.012)	(0.051)
N	32,164	32,164	32,103	32,103	32,164	32,164	32,103	32,103
Bandwidth	27.22	16.68	12.1	19.04	19.17	18.17	17.14	13.69

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1972 compulsory schooling reforms on alcohol and drug related hospital admissions, as outlined in equation (4), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table 5: Effect of 1972 Compulsory Schooling Reform on Cancer Diagnoses

	Men				Women			
	Any Cancer	Lung Cancer	Skin Cancer	Urin. Cancer	Any Cancer	Lung Cancer	Skin Cancer	Genital Cancers
Mean	0.080	0.007	0.025	0.019	0.142	0.007	0.024	0.028
(SD)	(0.272)	(0.081)	(0.157)	(0.138)	(0.350)	(0.084)	(0.153)	(0.164)
Coef	−0.048***	0.004	−0.011	−0.008**	−0.02	−0.006*	−0.005	−0.011*
(SE)	(0.013)	(0.003)	(0.007)	(0.004)	(0.019)	(0.003)	(0.007)	(0.006)
N	31,776	31,776	31,776	31,776	31,720	31,720	31,720	31,568
Bandwidth	15.24	27.90	28.75	20.11	23.24	16.47	34.80	24.05

Notes: ***/**/* indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1972 compulsory schooling reform on cancer diagnoses as outlined in equation (4), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

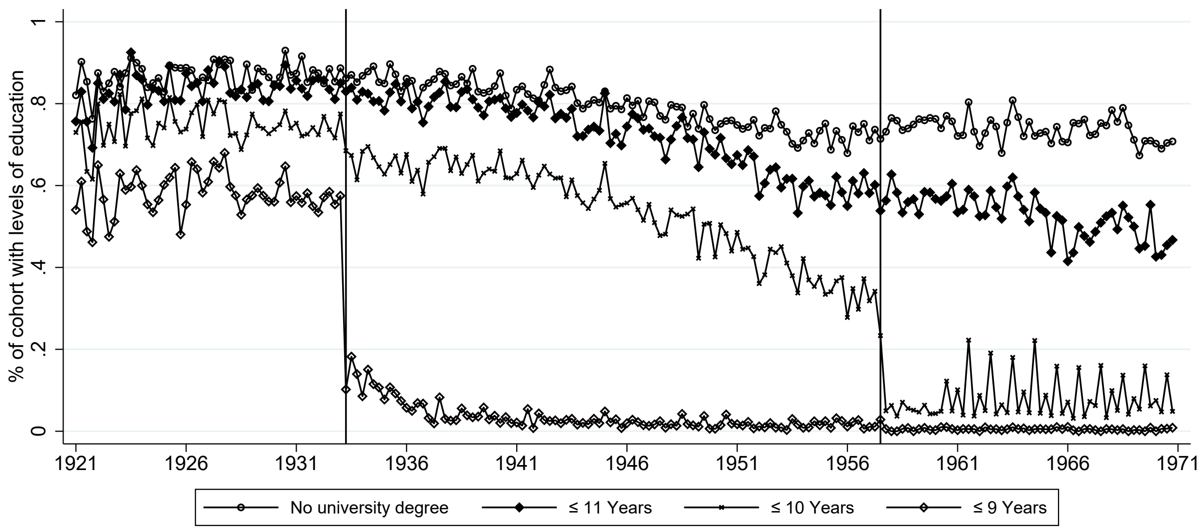
Table 6: Effect of 1972 ROSLA on Socio-Economic Outcomes

	Skilled Occ.	Most Deprived	Below Median	Least Deprived
Panel A: Men				
Mean	0.755	0.095	0.489	0.108
(SD)	(0.430)	(0.294)	(0.500)	(0.310)
Coef	0.001	0.007	−0.007	−0.015
(SE)	(0.012)	(0.011)	(0.024)	(0.011)
N	31,776	31,745	31,745	31,745
Bandwidth	16.67	28.73	18.54	21.76
Panel B: Women				
Mean	0.728	0.099	0.488	0.111
(SD)	(0.445)	(0.299)	(0.500)	(0.314)
Coef	0.021	−0.007	0.042***	−0.010
(SE)	(0.020)	(0.008)	(0.012)	(0.010)
N	31,720	31,711	31,711	31,711
Bandwidth	21.75	17.25	22.49	28.82

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of compulsory schooling reforms on a set of socio-economic outcomes, in particular whether a SLS participant reports to be employed in either a professional, managerial, technical, or other skilled occupation; whether a SLS participant reports living in one of the 10% most deprived neighborhoods, in a neighborhood below deprivation median, and one of the 10% least deprived neighborhoods (as measured by the Scottish Index of Multiple Deprivations (SMID). Estimation is outlined in equation (4) and uses local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of a ROSLA cut-off date were included in the analysis.

Figure 1: Years of Full-Time Education by Quarter of Birth

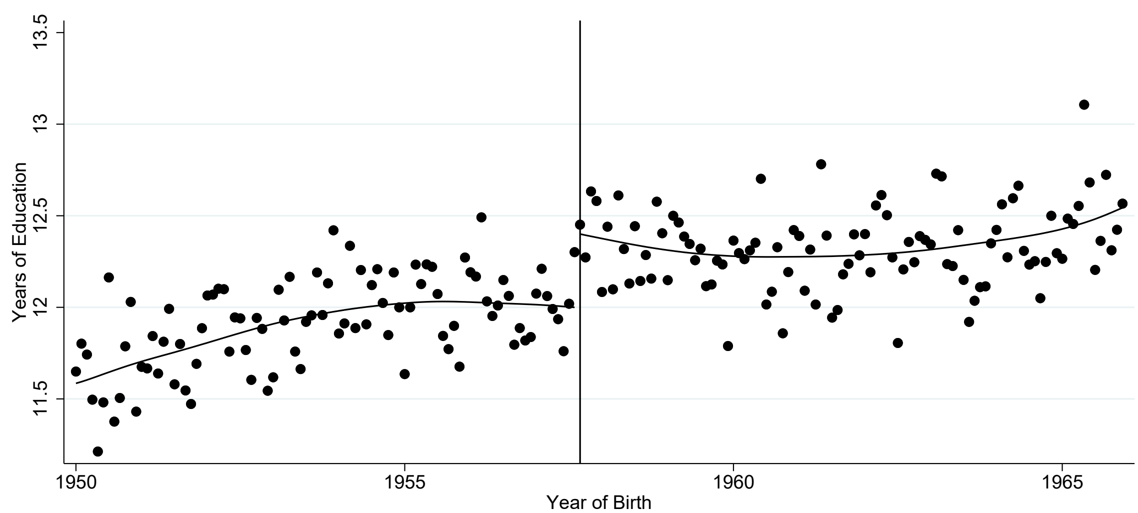


Notes: Each dot is a quarter of birth cohort. Vertical lines indicate ROSLA reforms. The 1947 reform is indicated by the first vertical line and increased the minimum schooling age from 14 to 15 for all cohorts born in the 2nd quarter of 1933 or after. The second reform increased the minimum school leaving age from 15 to 16 for all cohorts born in or after September 1957.
Data Source: Scottish Health Surveys of 1995-2016

Figure 2: Effect of 1972 ROSLA Reform on Educational Attainment

(a) ≤10 Years of Education

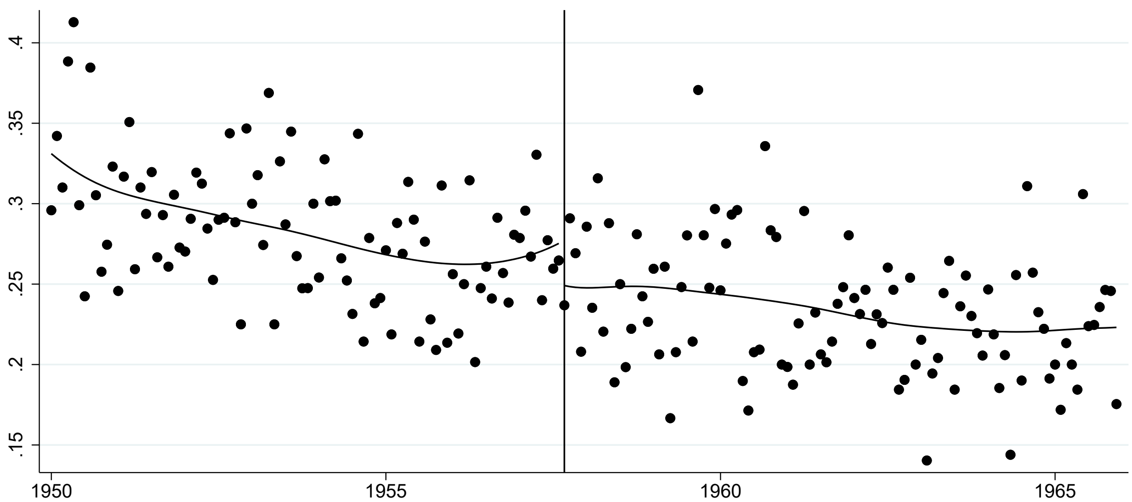
(b) Years of Education



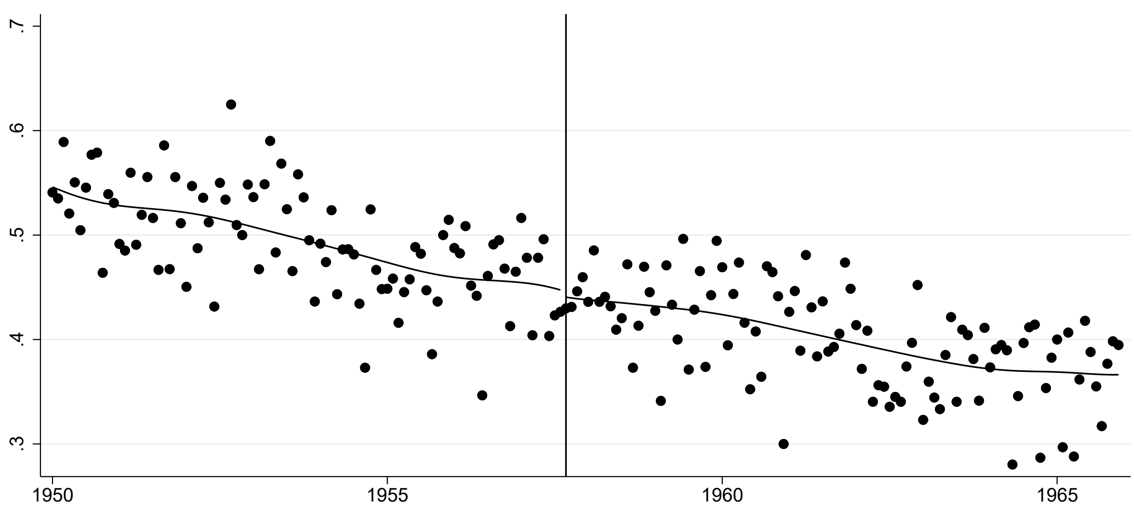
Notes: Each dot is the average of a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reform. Data Source: Pooled data from 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure 3: Effect of 1972 ROSLA Reform on Self-Reported Health

(a) 1972 Reform: Poor Health

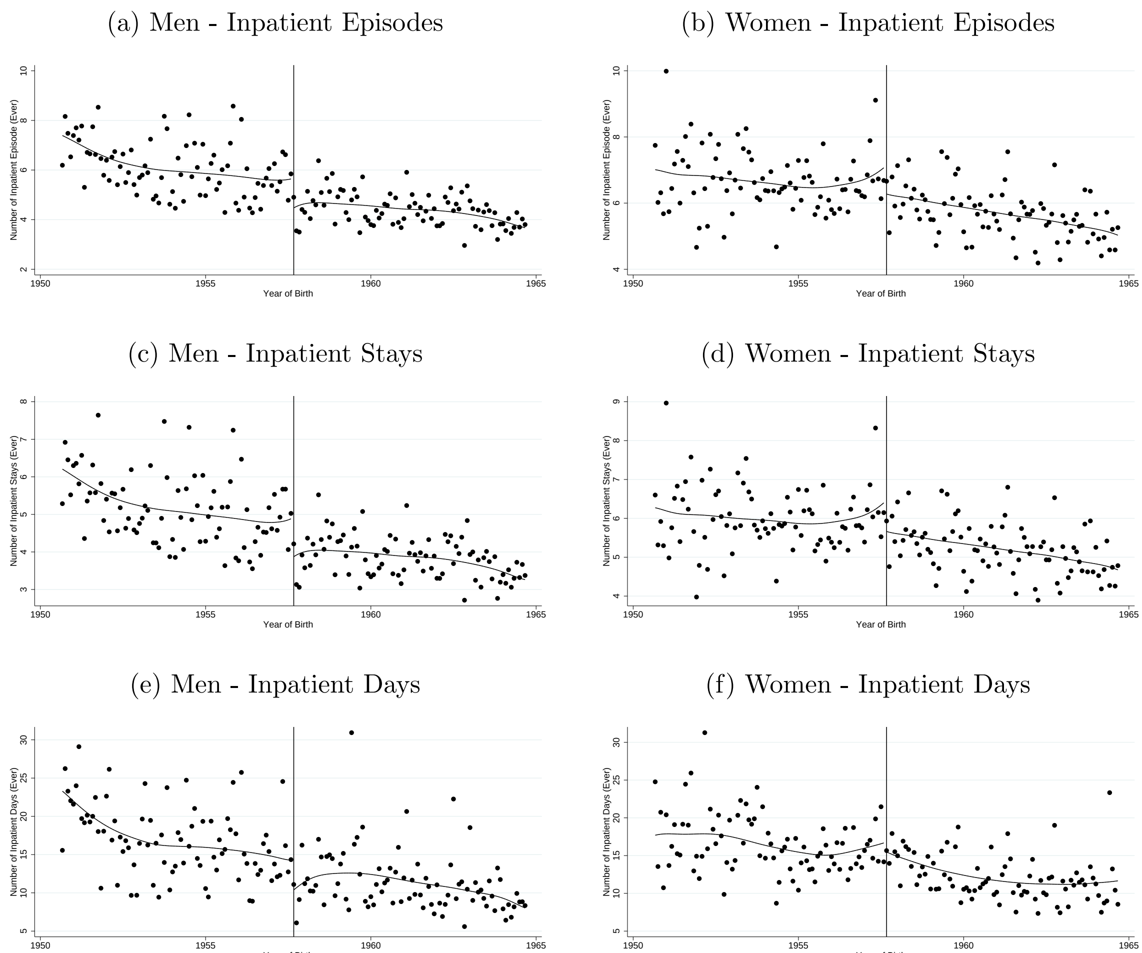


(b) 1972 Reform: Long-Standing Illness



Notes: Each dot shows the fraction of a month-year birth cohort who reported to be in fair, bad, or very bad health and who reported having a long-standing illness, respectively. Horizontal lowess lines provide a flexible fit. Vertical lines indicate 1972 ROSLA reform. All figures are constructed by pooling the 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure 4: Effect of 1972 ROSLA Reform on Inpatient Hospitalizations



(a) Men - Inpatient Episodes

(b) Women - Inpatient Episodes

(c) Men - Inpatient Stays

(d) Women - Inpatient Stays

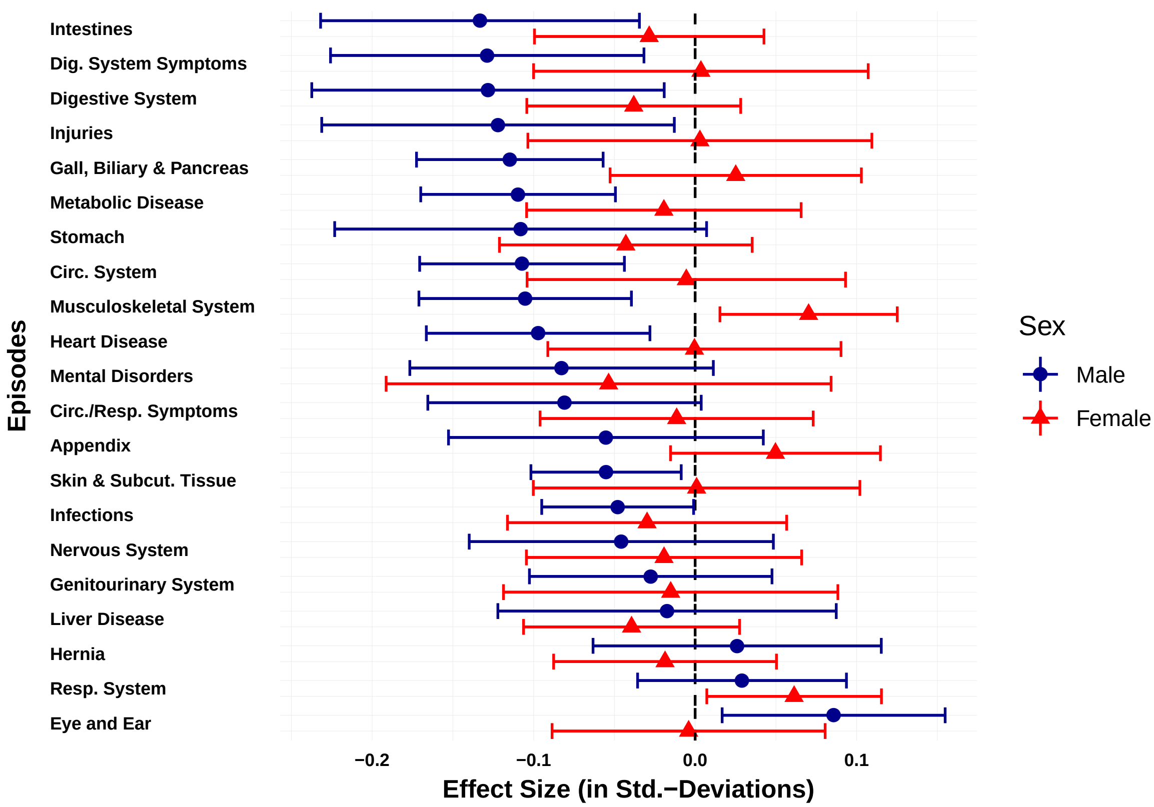
(e) Men - Inpatient Days

(f) Women - Inpatient Days

Notes: Each dot shows the the average number of inpatient episodes, stays, and days for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure 5: Effect of 1972 ROSLA on Inpatient Episodes by Diagnosis

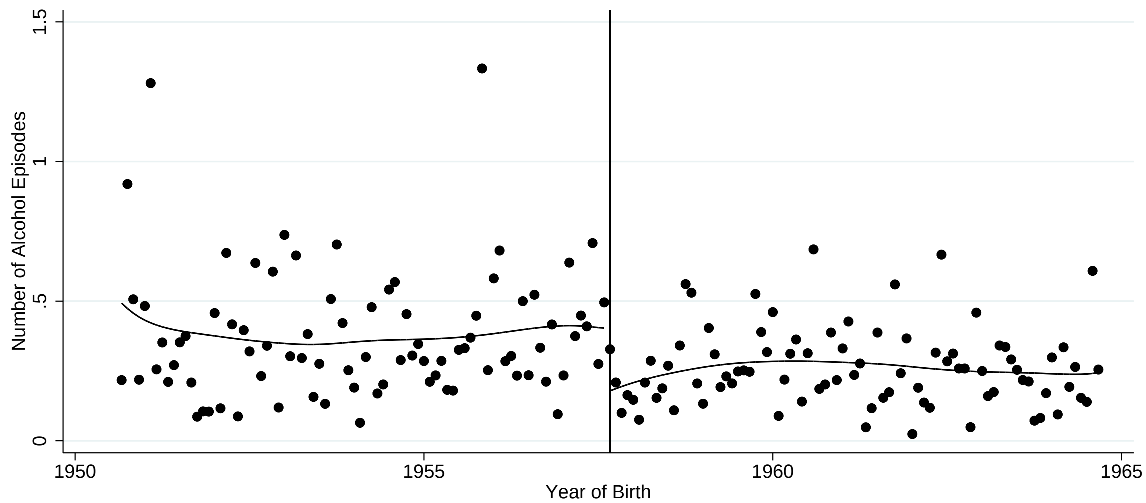
(a) Effect on Inpatient Care Episodes



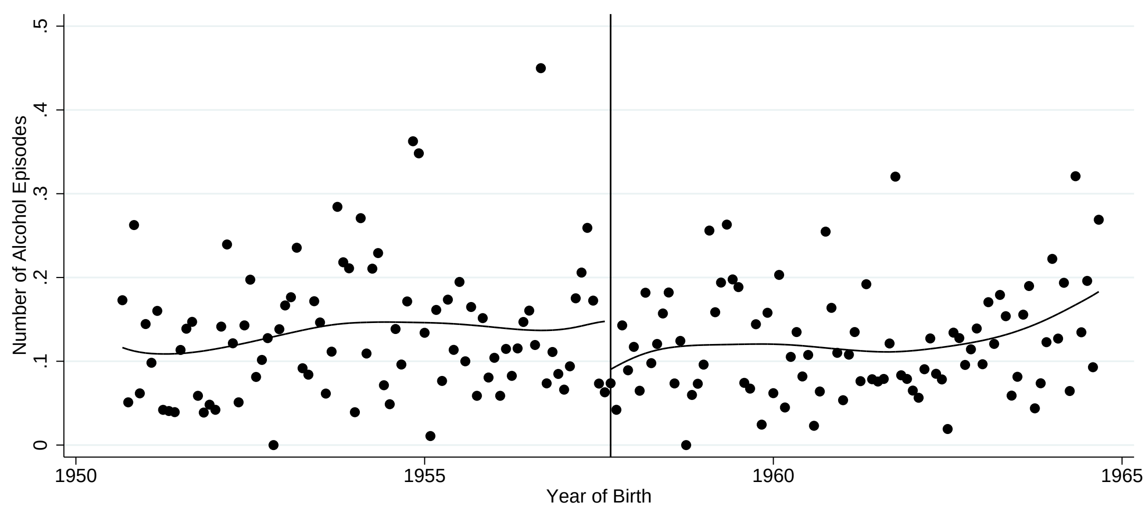
Notes: Forrest plot shows reduced form point estimates and 95% confidence intervals for the effect of the 1972 ROSLA reform on the number of inpatient hospitalization episodes. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure 6: Effect of 1972 ROSLA Reform on Alcohol-Related Hospitalizations

(a) Number of Episodes - Men

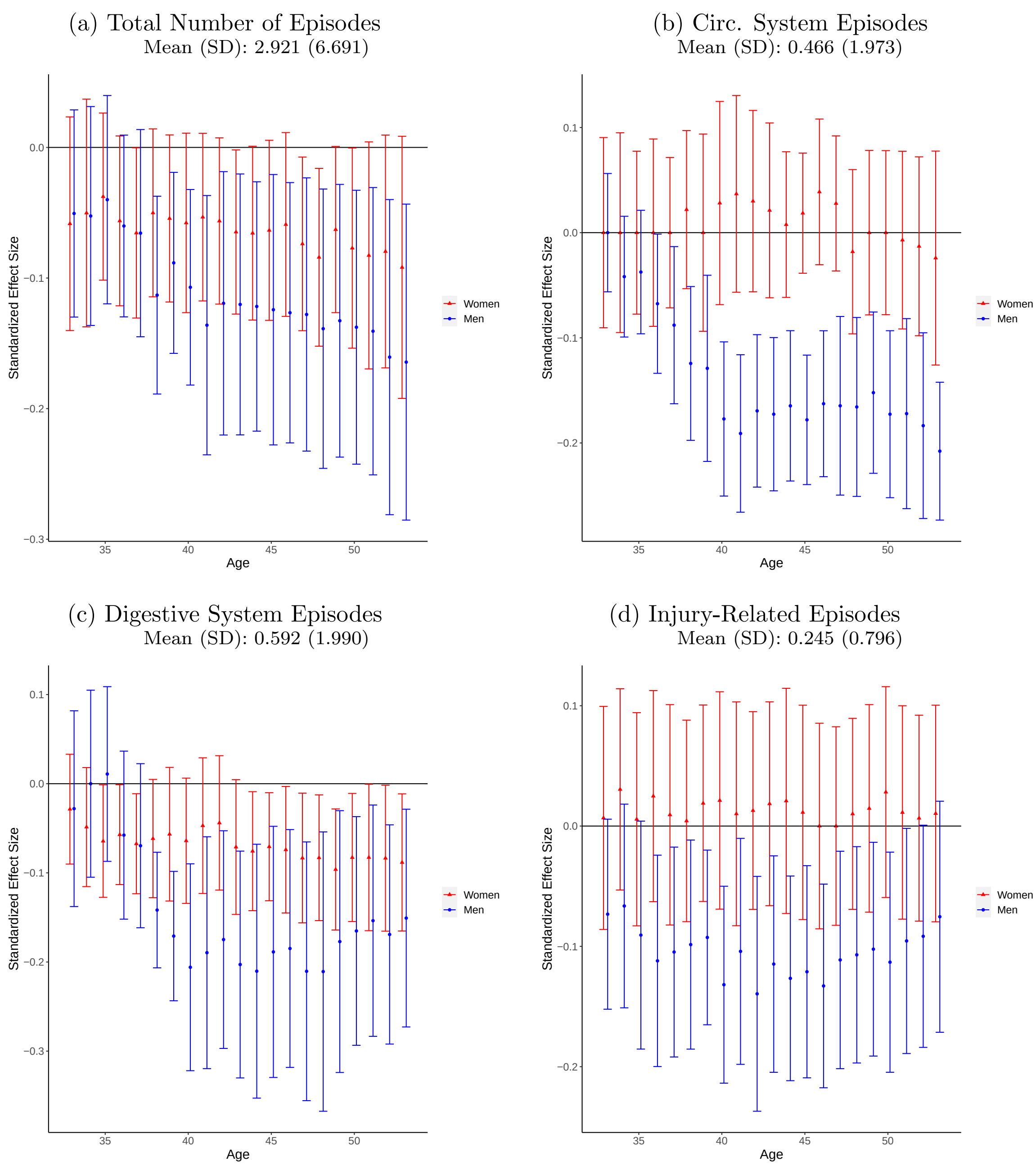


(b) Number of Episodes - Women



Notes: Each dot shows the average number of inpatient episodes due to alcohol poisoning, intoxication, harmful use, or dependency/withdrawal for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate 1972 ROSLA reform. Data Source: Scottish Longitudinal Study (SLS).

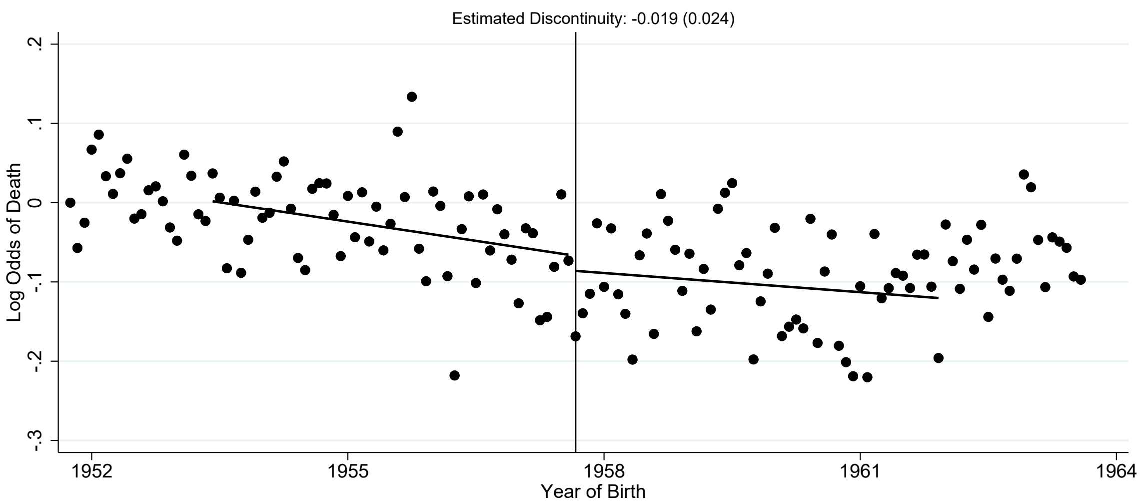
Figure 7: Life-Cycle Effects of 1972 ROSLA Reforms on Hospitalizations



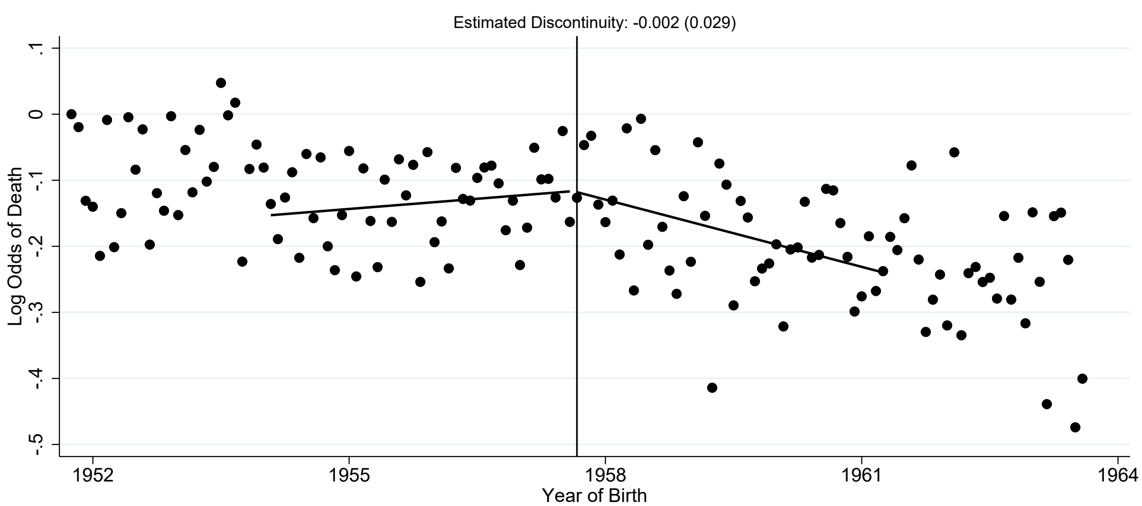
Notes: Charts shows reduced form point estimates and 95% confidence intervals for the effect of the 1972 ROSLA reform on the number of inpatient hospitalization episodes in general and by select conditions. Each estimate describes the effect of the 1972 ROSLA on the number of hospitalizations by a particular age. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex. Reported means and standard deviations refer to men. Data Source: Scottish Longitudinal Study (SLS).

Figure 8: Effect of 1972 ROSLA Reform on Mortality

(a) Men



(b) Women



Notes: Each dot shows the log odds death ratio for a month-year birth cohort. All estimates should be interpreted relative to the September 1950 cohort. Estimates are obtained using a two-step procedure outlined in section 5.4.1. In a first step, a panel logit approach regresses mortality on birthmonth/year fixed effects. The fitted values of these fixed effects become the outcome in the second step which uses local linear regressions to estimate a discontinuity around the 1972 ROSLA reform cutoff. Data Source: Scottish Census and Death Registry.

Appendix A

Table A1: Table of Means for SLS Samples

Panel A: Sample for 1947 ROSLA	Men		Women	
	April 1926 - March 1933	April 1933 - March 1940	April 1926 - March 1933	April 1933 - March 1940
Pre-Defined Characteristics				
% White	0.998	0.997	0.996	0.997
% Non-White	0.002	0.003	0.004	0.003
% Catholic (Raised)	0.089	0.115	0.104	0.131
% Church of Scotland (Raised)	0.450	0.496	0.469	0.502
Education Outcomes				
% No Qualification	0.746	0.663	0.764	0.677
%At Least O-Grades	0.254	0.337	0.236	0.323
% Degree	0.115	0.140	0.104	0.128
Post-Defined Characteristics				
% Unskilled	0.273	0.243	0.350	0.338
% Skilled	0.439	0.443	0.398	0.395
% Professional	0.288	0.315	0.252	0.267
% Ever Married	0.914	0.912	0.912	0.937
Deprivation Index	−0.056	−0.146	−0.027	−0.150
N	9,910	11,240	11,408	11,801

Panel B: Sample for 1972 ROSLA	Men		Women	
	Sep. 1950 - August 1957	Sep. 1957 - August 1964	Sep. 1950 - August 1957	Sep. 1957 - August 1964
Pre-Defined Characteristics				
% White	0.998	0.997	0.998	0.998
% Non-White	0.002	0.003	0.002	0.002
% Catholic (Raised)	0.139	0.149	0.16	0.173
% Church of Scotland (Raised)	0.447	0.386	0.475	0.427
Education Outcomes				
% No Qualification	0.371	0.296	0.377	0.269
% At Least O-Grades	0.629	0.704	0.623	0.731
% Degree	0.245	0.239	0.256	0.255
Post-Defined Characteristics				
% Unskilled	0.193	0.240	0.259	0.242
% Skilled	0.447	0.474	0.418	0.457
% Professional	0.36	0.287	0.324	0.301
% Ever Married	0.843	0.665	0.894	0.759
Deprivation Index	−0.486	−0.155	−0.332	0.024
N	14,641	17,135	14,774	16,946

Notes: This table shows the means for all observable demographic characteristics of SLS participants who were born within seven years of either ROSLA cut-off date. Data Source: Scottish Longitudinal Study (SLS).

Table A2: First Stage Results: Effect of 1947 Compulsory Schooling Reform on Education

	Years of Education	≤9 years	≤10 years	≤11 years	≤12 years	≤13 years
Panel A: Impact of 1947 ROSLA						
	All (N=37,078; bandwidth = 54 months)					
Estimate	0.525***	−0.413***	−0.070***	−0.015	0.011	−0.007
	(0.111)	(0.024)	(0.023)	(0.018)	(0.015)	(0.016)
Outcome Mean	10.17	0.571	0.749	0.838	0.891	0.925
	Men (N=16,500; bandwidth = 49 months)					
Estimate	0.359***	−0.402***	−0.014	0.030	0.024	0.001
	(0.136)	(0.038)	(0.031)	(0.029)	(0.022)	(0.020)
Outcome Mean	10.34	0.555	0.712	0.806	0.866	0.908
	Women (N=20,578; bandwidth = 54 months)					
Estimate	0.655***	−0.413***	−0.109***	−0.037*	−0.0359*	−0.015
	(0.148)	(0.035)	(0.035)	(0.022)	(0.019)	(0.021)
Outcome Mean	10.05	0.584	0.777	0.863	0.910	0.938

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the first stage effects of compulsory schooling reforms on educational attainment, using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table A3: Effect of Education on Self-Reported Health - 1947 ROSLA

	Poor Health	Illness	Current Drinker	Current Smoker	Ever Smoked
	All (N=37,071; bandwidth = 51 months)				
Mean	0.425	0.661	0.766	0.183	0.634
(SD)	(0.495)	(0.473)	(0.424)	(0.387)	(0.482)
Reduced Form	0.029	0.008	0.004	0.003	-0.005
	(0.025)	(0.025)	(0.020)	(0.014)	(0.030)
OLS (years of schooling)	-0.042***	-0.018***	0.017***	-0.032***	-0.021***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
OLS (>11 years)	-0.179***	-0.087***	0.078***	-0.130***	-0.096***
	(0.005)	(0.006)	(0.004)	(0.005)	(0.006)
	Men (N=16,498; bandwidth = 57 months)				
Mean	0.425	0.662	0.840	0.168	0.740
(SD)	(0.495)	(0.474)	(0.367)	(0.374)	(0.439)
Reduced Form	0.037	-0.020	-0.010	-0.004	-0.020
	(0.034)	(0.027)	(0.026)	(0.019)	(0.028)
OLS (years of schooling)	-0.040***	-0.018***	0.012***	-0.030***	-0.021***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
OLS (>11 years)	-0.178***	-0.090***	0.56***	-0.125***	-0.091***
	(0.008)	(0.008)	(0.004)	(0.005)	(0.008)
	Women (N=20,573; bandwidth = 63 months)				
Mean	0.424	0.661	0.709	0.194	0.554
(SD)	(0.495)	(0.474)	(0.455)	(0.396)	(0.498)
Reduced Form	0.021	0.030	0.019	-0.004	0.001
	(0.034)	(0.032)	(0.028)	(0.023)	(0.034)
OLS (years of schooling)	-0.043***	-0.018***	0.022***	-0.034***	-0.021***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
OLS (>11 years)	-0.180***	-0.085***	0.096***	-0.134***	-0.100***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.008)

Notes: $* * */ * */ *$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the the effects of education on self-reported health outcomes. OLS estimates refer to the effect of an additional year of schooling or of having more than 10 years of education. Reduced from effects yield the effect of the compulsory schooling reform on educational attainment and are obtained using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table A4: Effect of 1947 Compulsory Schooling Reform on Inpatient Hospitalizations

Panel A: All Hospitalizations

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	11.27	8.792	57.457	10.562	8.273	58.961
(SD)	(13.124)	(10.837)	(111.037)	(12.007)	(9.624)	(112.2)
Coef	-2.151^{**}	-1.849^{**}	-7.462	0.410	0.308	4.152
(SE)	(0.905)	(0.728)	(5.467)	(0.770)	(0.561)	(6.348)
N	21,311	21,311	21,311	23,407	23,407	23,407
Bandwidth	17.38	16.68	20.27	33.90	34.73	26.84

Panel B: Hospitalizations between Ages 53 and 77

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	6.302	5.263	18.395	5.310	4.535	18.174
(SD)	(8.882)	(7.448)	(48.075)	(7.533)	(6.258)	(47.285)
Coef	-1.195	-1.374^{*}	-3.601	0.130	0.149	-1.392
(SE)	(0.886)	(0.801)	(2.766)	(0.464)	(0.358)	(2.843)
N	12,813	12,813	12,813	16,796	16,796	16,796
Bandwidth	19.09	17.48	18.49	32.25	32.50	20.14

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effect of the 1947 compulsory schooling reform on the number of inpatient hospitalizations as outlined in equation (4), using local polynomial regression discontinuity estimation. In Panel A the outcome is the number of hospitalizations at any age between 1981 and 2016. In Panel B, the outcome is the number of hospitalizations between ages 53 to 77. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table A5: Effect of 1947 Compulsory Schooling Reform on Cancer Diagnoses

	Men				Women			
	Any Cancer	Lung Cancer	Skin Cancer	Urin. Cancer	Any Cancer	Lung Cancer	Skin Cancer	Genital Cancers
Mean	0.396	0.074	0.115	0.103	0.343	0.054	0.093	0.034
(SD)	(0.489)	(0.262)	(0.319)	(0.304)	(0.475)	(0.225)	(0.290)	(0.180)
Coef	−0.005	0.017	0.01	−0.042***	0.015	0.008	0.008	−0.001
(SE)	(0.025)	(0.016)	(0.016)	(0.013)	(0.032)	(0.014)	(0.020)	(0.010)
N	21,150	21,150	21,150	21,150	23,209	23,209	23,209	23,209
Bandwidth	32.07	22.53	29.07	18.26	33.56	24.18	29.61	21.69

Notes: $*\,*\,*/\,*\,*/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1947 compulsory schooling reform on cancer diagnoses as outlined in equation (4), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table A6: Effects of 1947 ROSLA on Alcohol and Drug-Related Hospitalizations

	Alcohol-Related Inpatient Admissions				Drug-Related Inpatient Admissions			
	Men		Women		Men		Women	
	Episodes	Days	Episodes	Days	Episodes	Days	Episodes	Days
Mean	0.283	2.187	0.127	0.975	0.033	0.007	0.049	0.008
(SD)	(1.325)	(15.832)	(0.844)	(8.824)	(0.983)	(0.113)	(1.229)	(0.134)
Coef	0.029	−0.729	0.114*	0.877	0.005	0.042	0.005	0.026
(SE)	(0.106)	(1.053)	(0.062)	(0.703	(0.004)	(0.047)	(0.004)	(0.037)
N	21,311	21,311	23,407	23,407	21,311	21,311	23,407	23,407
Bandwidth	24.49	37.72	12.63	14.56	16.37	20.58	18.51	17.59

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1947 compulsory schooling reform on alcohol and drug related hospital admissions, as outlined in equation (4), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table A7: Effect of Compulsory Schooling Reforms on Emigration

	1947 ROSLA		1972 ROSLA	
	Men	Women	Men	Women
Mean	0.022	0.019	0.048	0.039
(SD)	(0.146)	(0.138)	(0.213)	(0.193)
Coef	0.009*	0.007	0.009	−0.008
(SE)	(0.005)	(0.005)	(0.008)	(0.009)
N	21,484	23,492	35,228	34,831
Bandwidth	11.18	21.38	22.63	14.36

Notes: ∗∗∗/∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of compulsory schooling reforms on observed emigration. Unobserved emigration by definition cannot be assessed. The regression follows equation (4), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of a ROSLA cut-off date were included in the analysis.

Table A8: Placebo Regressions: 1958 to 1972 Birth Cohorts

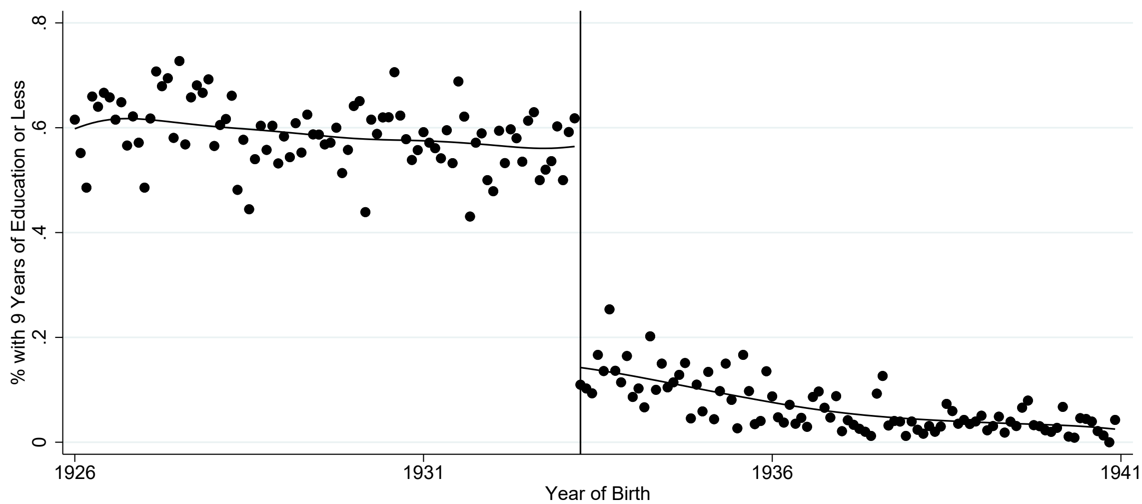
	No Qual.	Inpatient Epis. (all)	Inpatient Epis. (Digest. Syst.)	Alcohol-related Epis.
Panel A: Men				
Mean	0.287	3.876	0.750	0.244
(SD)	(0.452)	(7.438)	(2.440)	(1.756)
Coef	−0.019	−0.022	−0.066	0.038
(SE)	(0.016)	(0.284)	(0.082)	(0.068)
N	33,451	33,794	33,794	33,794
Bandwidth	26.66	25.60	26.88	26.34
Panel B: Women				
Mean	0.244	5.080	0.841	0.112
(SD)	(0.430)	(8.716)	(2.541)	(1.000)
Coef	−0.008	−0.527	0.003	−0.007
(SE)	(0.028)	(0.363)	(0.085)	(0.044)
N	33,564	33,802	33,802	33,802
Bandwidth	15.58	18.98	27.50	20.15

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

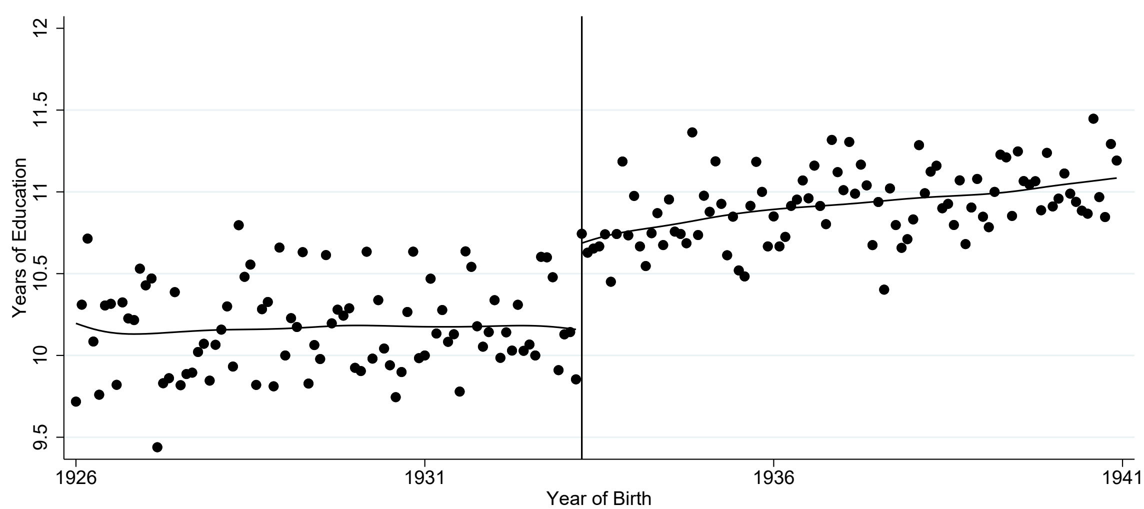
This table reports the results from a placebo regression in which only SLS respondents who were born between August 1958 and September 1972 were used. These cohorts were not subject to any ROSLA changes. The table calculates reduced form effects of a hypothetical ROSLA reform affecting everyone born after September 1965, as outlined in equation (4), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS).

Figure A1: Effect of 1947 ROSLA Reform on Educational Attainment

(a) ≤9 Years of Education



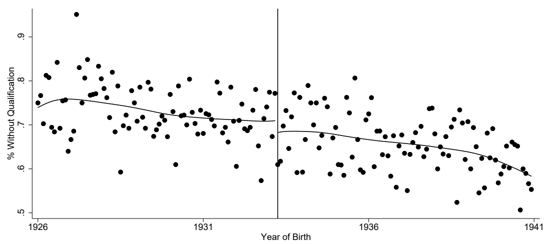
(b) Years of Education



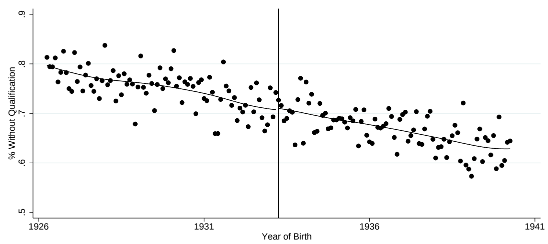
Notes: Each dot is the average of a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reform. Data Source: Pooled data from 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure A2: Comparison SHeS and SLS Data

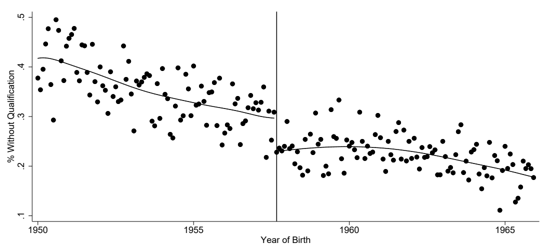
(a) SHeS - 1947 Reform: % No Qualification



(b) SLS - 1947 Reform: % No Qualification



(c) SHeS - 1972 Reform: % No Qualification



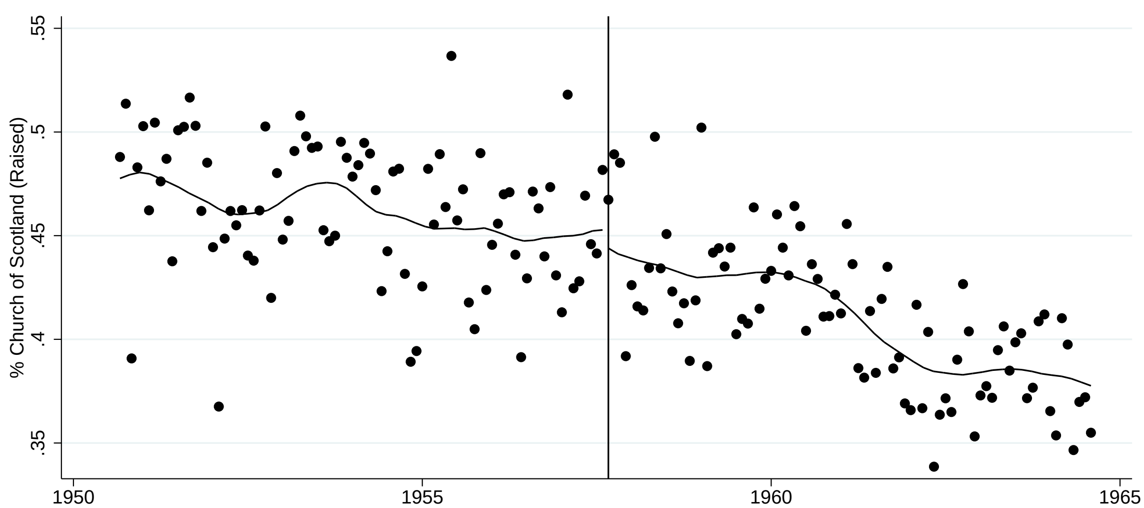
(d) SLS - 1972 Reform: % No Qualification

Notes: Each dot shows the fraction of a month-year birth cohort without formal qualification (≈ high school dropouts). Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Panels (a) and (c) are constructed by pooling the 1995-2016 waves of the Scottish Health Survey (SHeS). Panels (b) and (d) are constructed using each individual's most recent information in the Scottish Longitudinal Study (SLS).

Figure A3: Balanced Covariates: Religious Denomination

(a) 1947 Reform: Fraction Church of Scotland

(b) 1972 Reform: Fraction Church of Scotland



Notes: Each dot is the average of a month-year birth cohort that was raised Protestant (Church of Scotland). Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

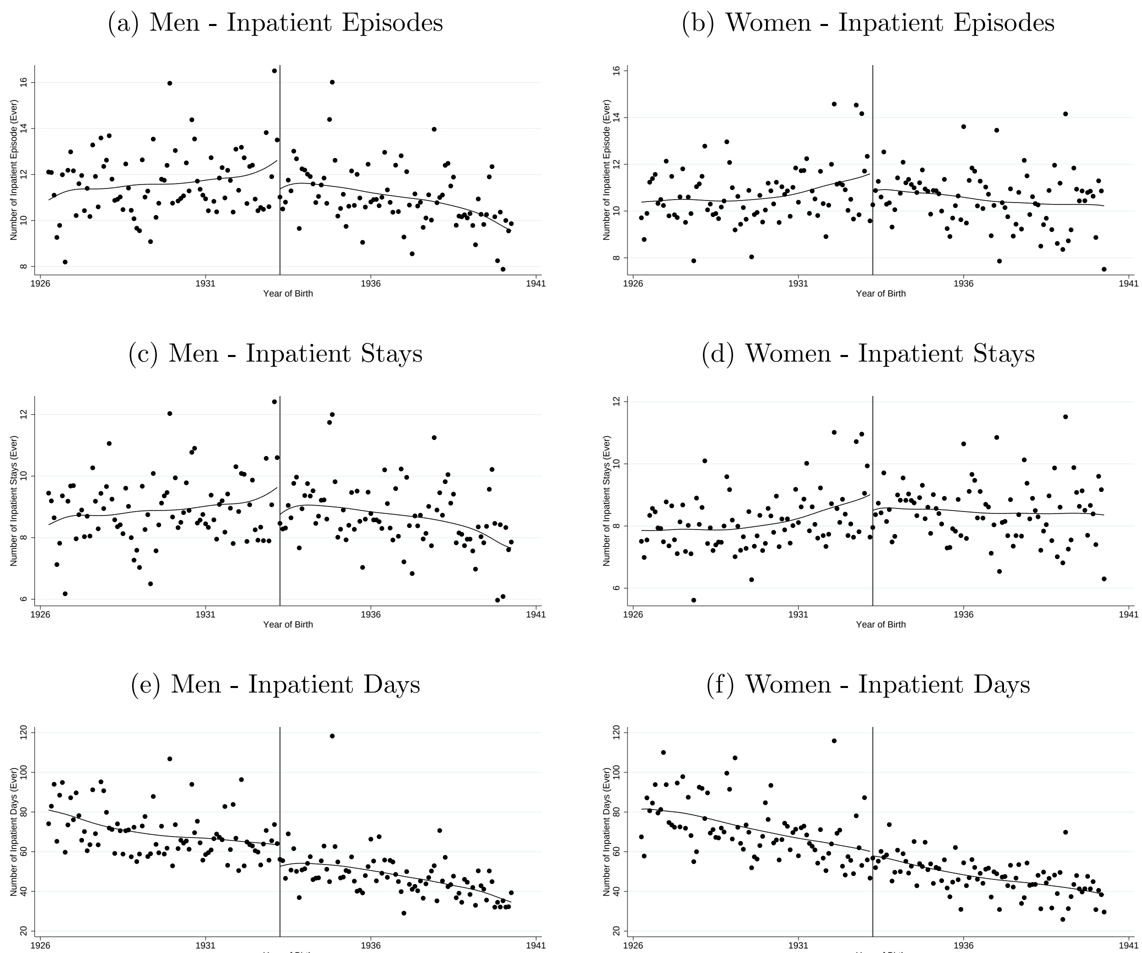
Figure A4: Effect of 1947 ROSLA Reform on Self-Reported Health

(a) 1947 Reform: Poor Health

(b) 1947 Reform: Long-Standing Illness

Notes: Each dot shows the fraction of a month-year birth cohort who reported to be in fair, bad, or very bad health and who reported having a long-standing illness, respectively. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. All figures are constructed by pooling the 1995-2016 waves of the Scottish Health Survey (SHeS).

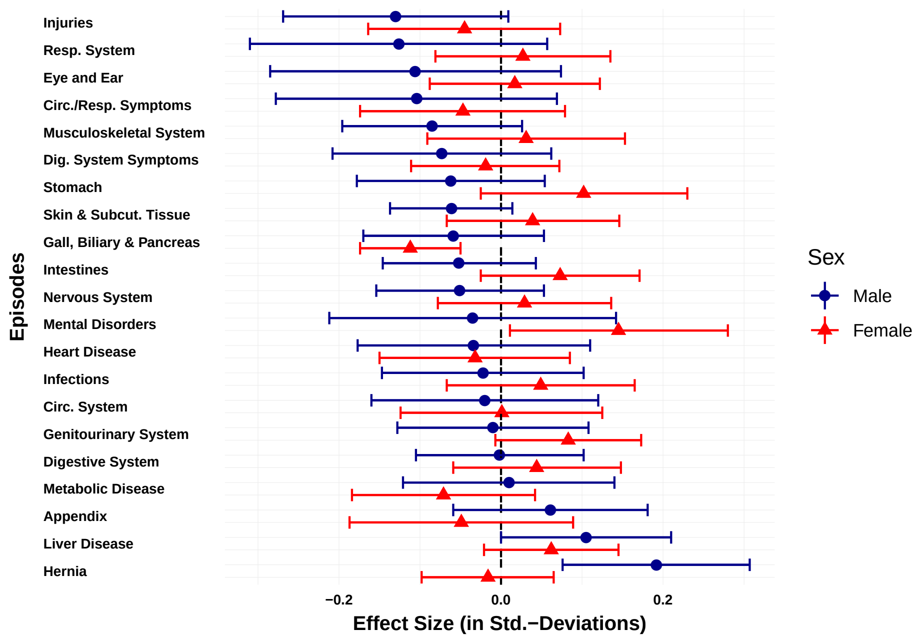
Figure A5: Effect of 1947 ROSLA Reform on Inpatient Hospitalizations



Notes: Each dot shows the the average number of inpatient episodes, stays, and days for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A6: Effect of 1947 ROSLA on Inpatient Episodes by Diagnosis

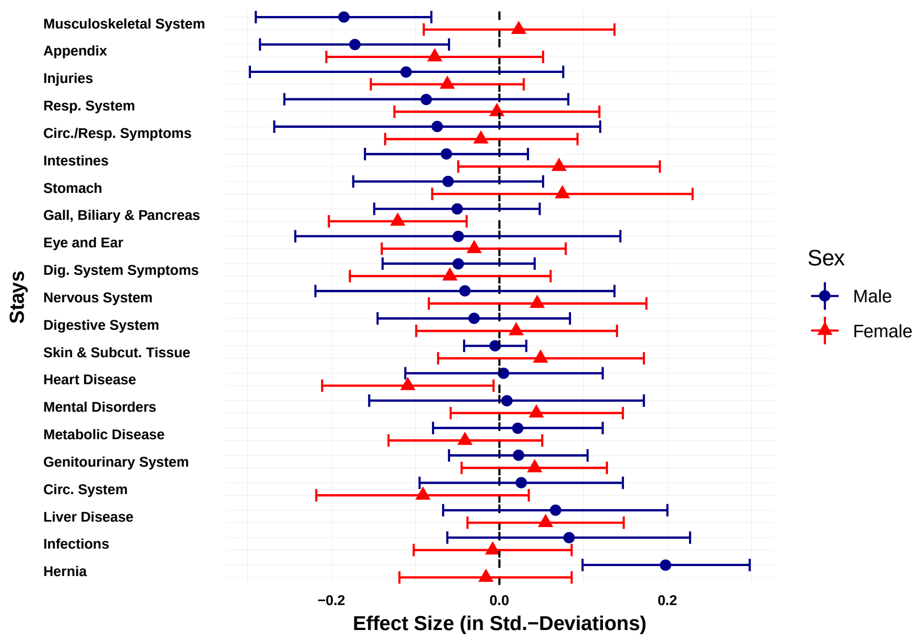
(a) Effect on Inpatient Care Episodes



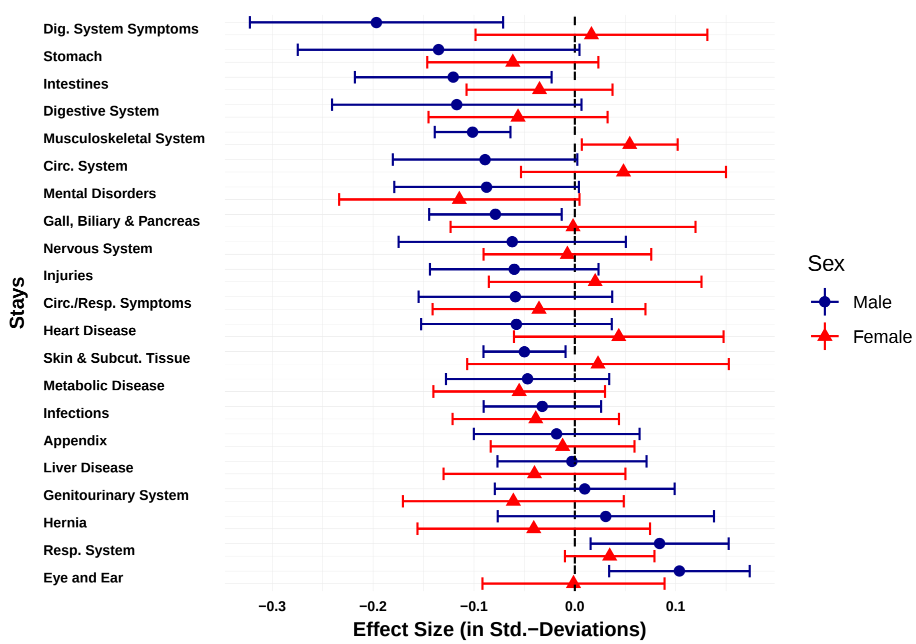
Notes: Forrest plot shows reduced form point estimates and 95% confidence intervals for the effect of the 1947 ROSLA reform on the number of inpatient hospitalization episodes. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure A7: Effect on Inpatient Stays by Diagnosis

(a) 1947 Reform: Effect on Stays in Inpatient Care



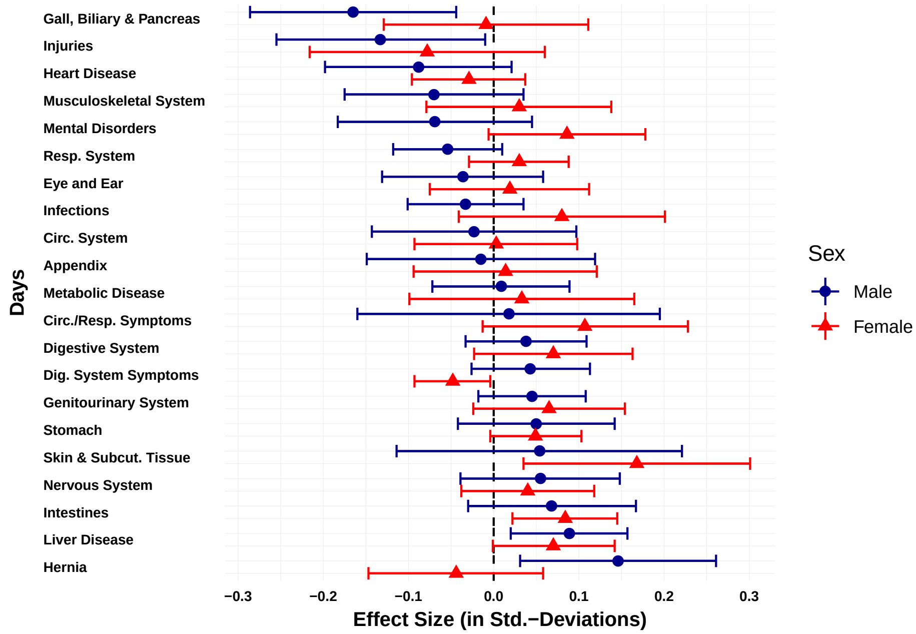
(b) 1972 Reform: Effect on Stays in Inpatient Care



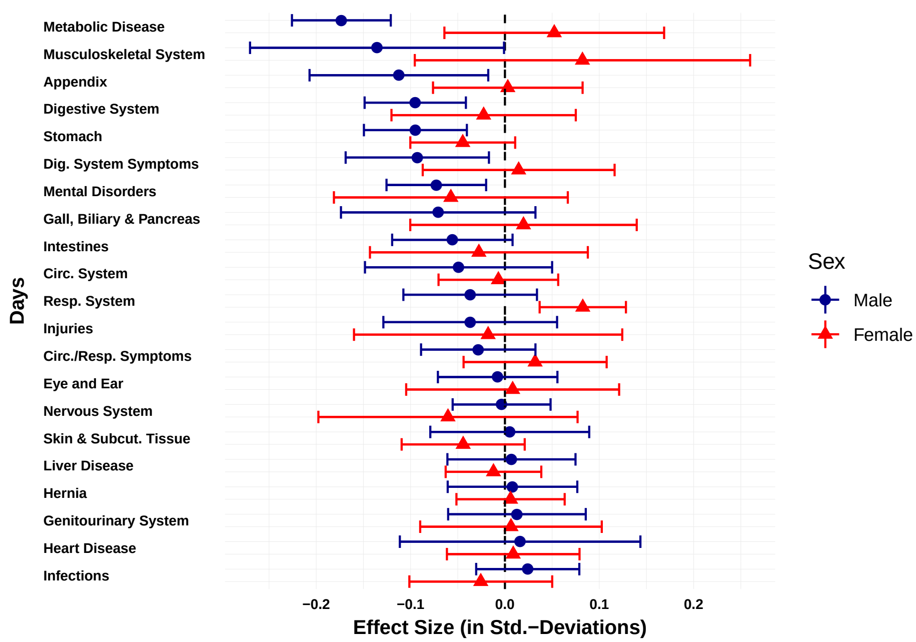
Notes: Forrest plots show reduced form point estimates and 95% confidence intervals for the effect of the ROSLA reforms on inpatient hospitalizations. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure A8: Effect on Inpatient Days by Diagnosis

(a) 1947 Reform: Effect on Days in Inpatient Care



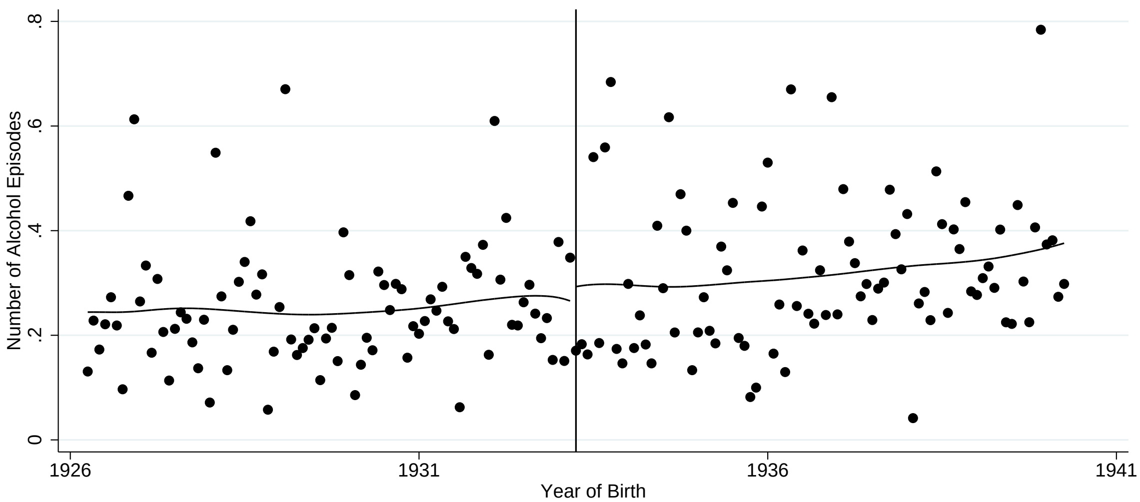
(b) 1972 Reform: Effect on Days in Inpatient Car



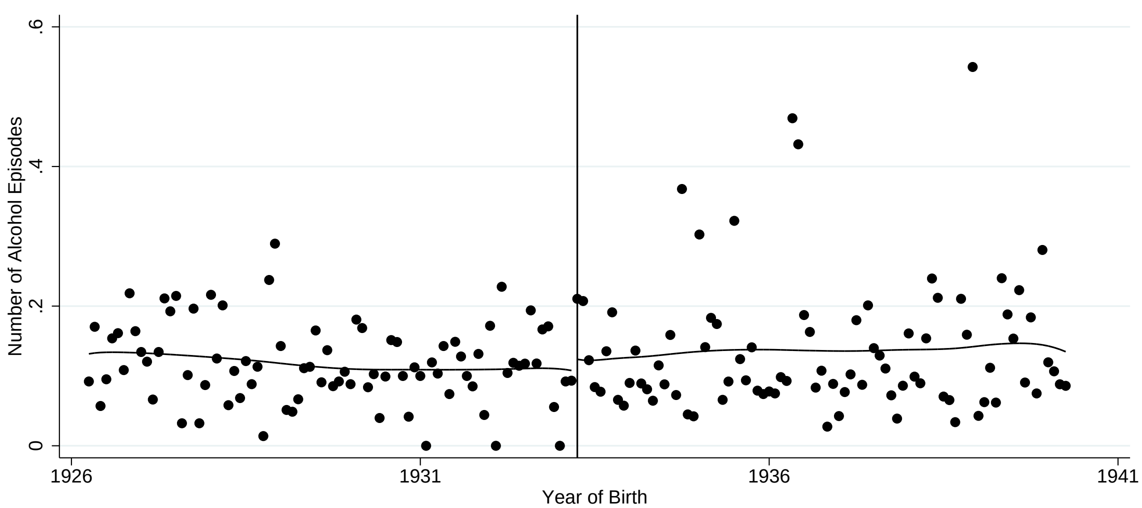
Notes: Forrest plots show reduced form point estimates and 95% confidence intervals for the effect of the ROSLA reforms on inpatient hospitalizations. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure A9: Effect of 1947 ROSLA Reform on Alcohol-Related Hospitalizations

(a) Number of Episodes - Men



(b) Number of Episodes - Women

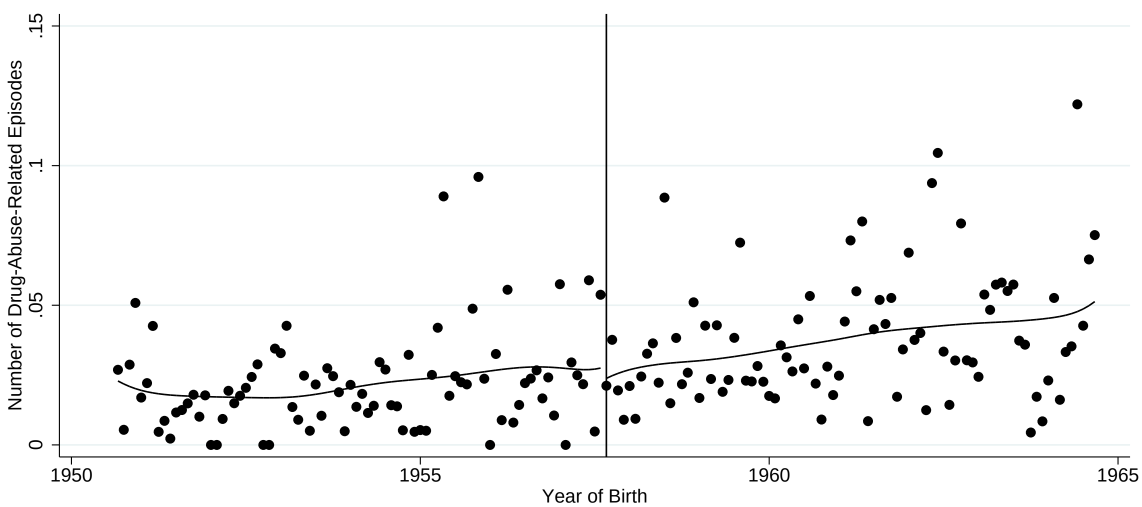


Notes: Each dot shows the average number of inpatient episodes due to alcohol poisoning, intoxication, harmful use, or dependency/withdrawal for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate 1947 ROSLA reform. Data Source: Scottish Longitudinal Study (SLS).

Figure A10: Drug Abuse Related Inpatient Hospital Episodes

(a) 1947 Reform: Drug Abuse Related Admissions

(b) 1972 Reform: Drug Abuse Related Admissions

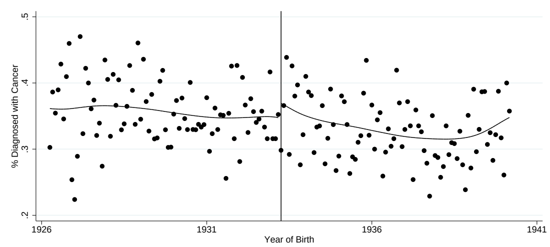


Notes: Each dot is the average number of drug abuse related admissions for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A11: Effect of ROSLA Reforms on Cancer Prevalence

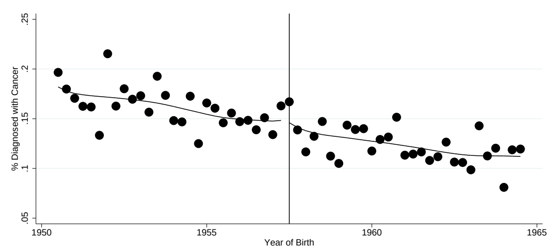
(a) 1947 Reform: Any Cancer Diagn. (Men)

(b) 1947 Reform: Any Cancer Diagn. (Women)



(c) 1972 Reform: Any Cancer Diagn. (Men)

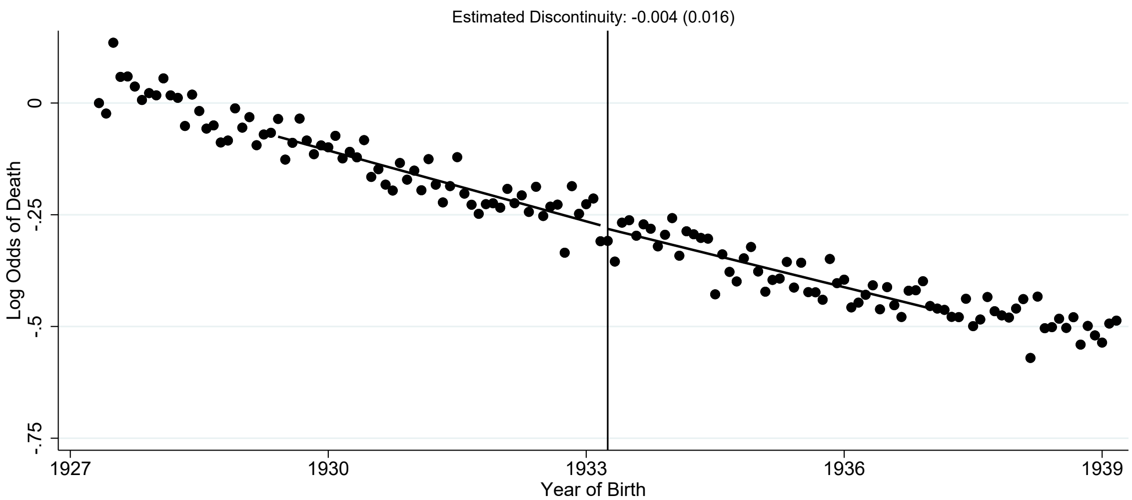
(d) 1972 Reform: Any Cancer Diagn. (Women)



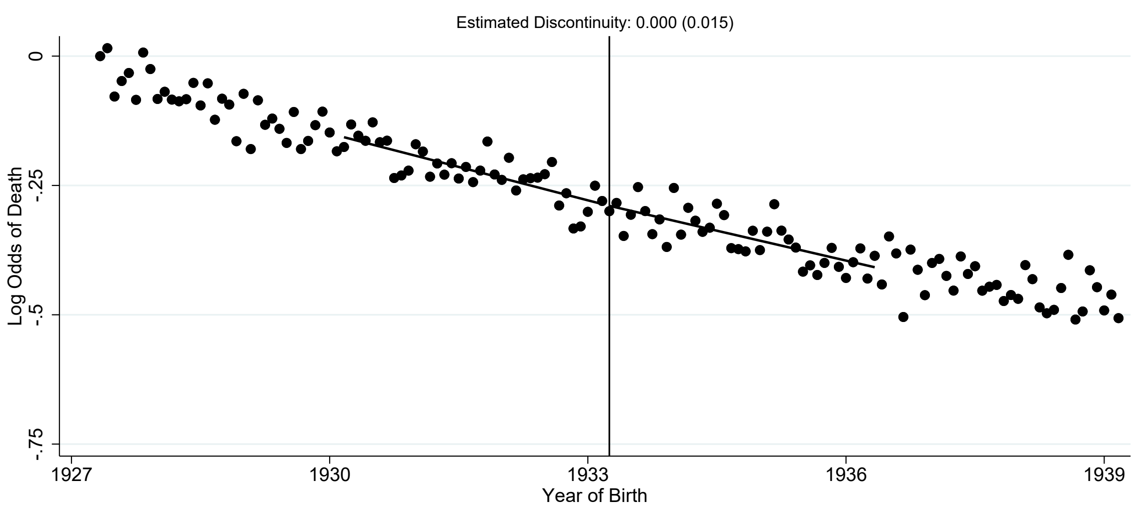
Notes: Each dot is the average of a month-year birth cohort that has been diagnosed with cancer. Because cancer diagnoses are rare for these groups, we show quarterly aggregates for later cohorts due to disclosure control reasons. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A12: Effect of 1947 ROSLA on Mortality

(a) Men



(b) Women



Notes: Each dot shows the log odds death ratio for a month-year birth cohort. All estimates should be interpreted relative to the April 1926 cohort. Data Source: Scottish Census and Death Registry.

A Appendix B

A.1 Documentation Hospitalization Categories

In this appendix, we provide detailed definitions of diagnosis subcategories. These categories are listed in Figure 5. The basis of our classification is International statistical classification of diseases and related health problems in its 10th revision, 5th edition (2016). Analyzed group categories are in bold, analyzed subcategories in italics:

Infectious and parasitic diseases - ICD10 Codes A00-B99

"Infections" include: Intestinal infectious diseases (A00-A09); Tuberculosis (A15-A19); Certain zoonotic bacterial diseases (A20-A28); Other bacterial diseases (A30-A49); Infections with a predominantly sexual mode of transmission (A50-A64); Other spirochetal diseases (A65-A69) Other diseases caused by chlamydiae (A70-A74); Rickettsioses (A75-A79); Viral and prion infections of the central nervous system (A80-A89); Arthropod-borne viral fevers and viral hemorrhagic fevers (A90-A99);Viral infections characterized by skin and mucous membrane lesions (B00-B09); Other human herpesviruses (B10); Viral hepatitis (B15-B19); Human immunodeficiency virus [HIV] disease (B20-B20); Other viral diseases (B25-B34); Mycoses (B35-B49); Protozoal diseases (B50-B64); Helminthiases (B65-B83); Pediculosis, acariasis and other infestations (B85-B89); Sequelae of infectious and parasitic diseases (B90-B94); Bacterial and viral infectious agents (B95-B97); Other infectious diseases (B99).

Endocrine, nutritional and metabolic diseases - ICD10 Codes E00-E89

"Metabolic Diseases" include: Disorders of thyroid gland (E00-E07); Diabetes mellitus (E08-E13); Other disorders of glucose regulation and pancreatic internal secretions (E15-E16); Disorders of other endocrine gland (E20-E35); Intraoperative complications of endocrine system (E36); Malnutrition (E40-E46); Other nutritional deficiencies (E50-E64); Overweight, obesity and other hyperalimentation (E65-E68); Metabolic disorders (E70-E88); Other postprocedural endocrine and metabolic complications and disorders (E89).

Mental, Behavioral and Neurodevelopmental disorders - ICD10 Codes F01-F99

“Mental Disorders” include: Mental disorders due to known physiological conditions (F01-F09); Mental and behavioral disorders due to psychoactive substance use (F10-F19); Schizophrenia, schizotypal, delusional, and other non-mood psychotic disorders (F20-F29); Mood [affective] disorders (F30-F39); Anxiety, dissociative, stress-related, somatoform and other nonpsychotic mental disorders (F40-F48); Behavioral syndromes associated with physiological disturbances and physical factors (F50-F59); Disorders of adult personality and behavior (F60-F69); Intellectual Disabilities (F70-F79); Pervasive and specific developmental disorders (F80-F89); Behavioral and emotional disorders with onset usually occurring in childhood and adolescence (F90-F98); Unspecified mental disorder (F99-F99).

Diseases of the nervous system - ICD10 Codes G00-G99

“Nervous System” include: Inflammatory diseases of the central nervous system (G00-G09); Systemic atrophies primarily affecting the central nervous system (G10-G14); Extrapyramidal and movement disorders (G20-G26); Other degenerative diseases of the nervous system (G30-G32); Demyelinating diseases of the central nervous system (G35-G37); Episodic and paroxysmal disorders (G40-G47); Nerve, nerve root and plexus disorders (G50-G59); Polyneuropathies and other disorders of the peripheral nervous system (G60-G65); Diseases of myoneural junction and muscle (G70-G73); Cerebral palsy and other paralytic syndromes (G80-G83); Other disorders of the nervous system (G89-G99).

Diseases of the eye and adnexa & Diseases of the ear and mastoid process - ICD10 Codes H00-H95

“Eye and Ear” include: Disorders of eyelid, lacrimal system and orbit (H00-H05); Disorders of conjunctiva (H10-H11); Disorders of sclera, cornea, iris and ciliary body (H15-H22);

Disorders of lens (H25-H28); Disorders of choroid and retina (H30-H36); Glaucoma (H40-H42); Disorders of vitreous body and globe (H43-H44); Disorders of optic nerve and visual pathways (H46-H47); Disorders of ocular muscles, binocular movement, accommodation and refraction (H49-H52); Visual disturbances and blindness (H53-H54); Other disorders of eye and adnexa (H55-H57); Intraoperative and postprocedural complications and other disorders of eye and adnexa (H59); Diseases of external ear (H60-H62); Diseases of middle ear and mastoid (H65-H75); Diseases of inner ear (H80-H83); Other disorders of ear (H90-H94); Other intraoperative and postprocedural complications and disorders of ear and mastoid process (H95).

Diseases of the circulatory system - ICD10 Codes I00-I99

"Circ. System" include: *Heart Disease: Acute rheumatic fever (I00-I02); Chronic rheumatic heart diseases (I05-I09); Hypertensive diseases (I10-I16);Ischemic heart diseases (I20-I25); Pulmonary heart disease and diseases of pulmonary circulation (I26-I28); Other forms of heart disease (I30-I52);* Cerebrovascular diseases (I60-I69); Diseases of arteries, arterioles and capillaries (I70-I79); Diseases of veins, lymphatic vessels and lymph nodes, not elsewhere classified (I80-I89); Other and unspecified disorders of the circulatory system (I95-I99).

Diseases of the respiratory system - ICD10 Codes J00-J99

"Resp. System" include: Acute upper respiratory infections (J00-J06); Influenza and pneumonia (J09-J18); Other acute lower respiratory infections (J20-J22); Other diseases of upper respiratory tract (J30-J39); Chronic lower respiratory diseases (J40-J47); Lung diseases due to external agents (J60-J70); Other respiratory diseases principally affecting the interstitium (J80-J84); Suppurative and necrotic conditions of the lower respiratory tract (J85-J86); Other diseases of the pleura (J90-J94); Intraoperative and postprocedural complications and disorders of respiratory system, not elsewhere classified (J95); Other diseases of the respiratory system (J96-J99).

Diseases of the digestive system - ICD10 Code: K00-K95

"Digestive System" include: Diseases of oral cavity and salivary glands (K00-K14); *Diseases of esophagus, stomach and duodenum (K20-K3) ("Stomach"); Diseases of appendix (K35-K38) ("Appendix"); Hernia (K40-K46) ("Hernia"); Noninfective enteritis and colitis (K50-K52); Other diseases of intestines (K55-K64) ("Intestines");* Diseases of peritoneum and retroperitoneum (K65-K68); *Diseases of liver (K70-K77) ("Liver Disease")*; *Disorders of gallbladder, biliary tract and pancreas (K80-K87) ("Gall, Biliary & Pancreas")*; Other diseases of the digestive system (K90-K95).

Diseases of the skin and subcutaneous tissue - ICD10 Codes L00-L99

"Skin & Subcut. Tissue" include: L00-L08Infections of the skin and subcutaneous tissue Bullous disorders (L10-L14); Dermatitis and eczema (L20-L30); Papulosquamous disorders (L40-L45); Urticaria and erythema (L49-L54); Radiation-related disorders of the skin and subcutaneous tissue (L55-L59); Disorders of skin appendages (L60-L75); Intraoperative and postprocedural complications of skin and subcutaneous tissue (L76); Other disorders of the skin and subcutaneous tissue (L80-L99).

Diseases of the musculoskeletal system and connective tissue - ICD10 Codes M00-M99

"Musculoskeletal System" include: Arthropathies (M00-M25); Dentofacial anomalies [including malocclusion] and other disorders of jaw (M26-M27); Systemic connective tissue disorders (M30-M36); Dorsopathies (M40-M54); Soft tissue disorders (M60-M79); Osteopathies and chondropathie (M80-M94); ther disorders of the musculoskeletal system and connective tissue (M95); Other intraoperative and postprocedural complications and disorders of musculoskeletal system (M96); Periprosthetic fracture around internal prosthetic joint (M97); Biomechanical lesions, not elsewhere classified (M99).

Diseases of the genitourinary system - ICD10 codes N00-N99

“Genitourinary System” include: Glomerular diseases (N00-N08); Renal tubulo-interstitial diseases (N10-N16); Acute kidney failure and chronic kidney disease (N17-N19); Other disorders of kidney and ureter (N20-N23); Urolithiasis (N25-N29); Other diseases of the urinary system (N30-N39); Diseases of male genital organs (N40-N53); Disorders of breast (N60-N65); Inflammatory diseases of female pelvic organs (N70-N77); Noninflammatory disorders of female genital tract (N80-N98); Other intraoperative and postprocedural complications and disorders of genitourinary system (N99)

Injury, poisoning and certain other consequences of external causes - ICD10 Codes S00-T88

“Injuries” include: Injuries to the head (S00-S09); Injuries to the neck (S10-S19); Injuries to the thorax (S20-S29); Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals (S30-S39); Injuries to the shoulder and upper arm (S40-S49); Injuries to the elbow and forearm (S50-S59); Injuries to the wrist, hand and fingers (S60-S69); Injuries to the hip and thigh (S70-S79); Injuries to the knee and lower leg (S80-S89); Injuries to the ankle and foot (S90-S99); Injury, poisoning and certain other consequences of external causes (T07-T88).

Symptoms and signs involving the circulatory and respiratory systems - ICD10 Codes R00-R09

“Circ./Resp. Symptoms.”

Symptoms and signs involving the digestive system and abdomen - ICD10 Codes R10-R19

“Dig. System Symptoms.”